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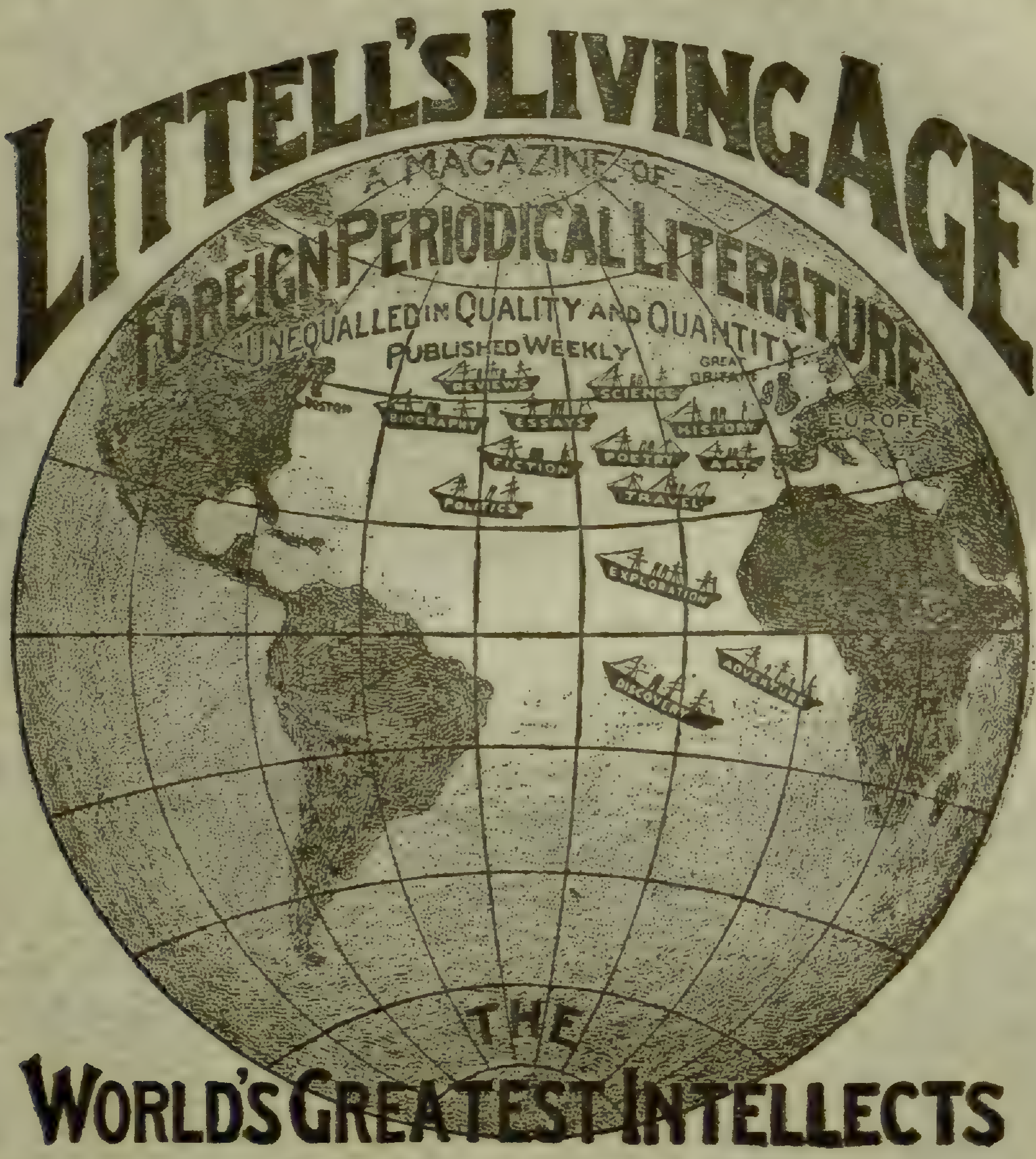

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ON HEREDITY AND REJUVENATION.[1]

By CHARLES SEDGWICK MINOT.[2]

The subject of this article is presented under the following sections:

I. The Formative Force of Organisms.
II. The Conception of Death.
III. A Comparison of Larva and Embryo.
IV. Concluding Remarks.

The first section is not new, but a reproduction without change, of an article published in *Science*, July 3d, 1885. As this article has not become generally known, and yet is an essential link in the chain of reasoning, I venture to repeat it. Though written in 1885, I consider that to-day it is still sufficient to disprove Weismann's theory of germ plasm. Weismann has not considered this article, otherwise, from my point of view, he could not have maintained his theory.

[1] This article is translated from one which appeared in the Biologisches Centralblatt, Vol. XV, Page 571, August 1st, 1895. A few trifling changes have been made in the text. An abstract of the article was read before the American Association for the Advancement of Science, at its recent Springfield meeting.

[2] Professor in the Harvard Medical School.

The views which I then defended have been recently brought forward in almost parallel form, and without essential additions, by O. Hertwig (*Zeit-und Streitfragen der Biologie*, I, Heft, D. 32–53) as arguments against the views of Weismann.

The second section is also directed against Weismann, for it attempts to replace his conception of death by one more exact.

The third section is intended to make the significance of rejuvenation clear, and at the same time, by a comparison of larvæ and embryos, to demonstrate a law of heredity which has not been hitherto recognized.

The Formative Force of Organisms.

The assertion is safe, that the majority of biologists incline at present to explain the forming of an organism out of its germ upon mechanical principles. The prevalent conception is that the forces of the ovum are so disposed that the evolution of the adult organism is the mechanical result of the predetermined interplay of those forces. The object of the present article is to point out that this conception is inadequate, and must be at least supplemented, if not replaced, by another view, namely, that the formative force is a generally diffused tendency, so that all parts inherently tend to complete by their own growth and modification the whole organism—a fact which finds a legitimate hypothetical expression in Darwin's Doctrine of pangenesis. The nature of the view here advanced will become clearer upon consideration of the evidence upon which it is based, and which is adduced below. The evidence that the formative force is diffused through all parts falls under three heads: 1. The process of regeneration in unicellular and multicellular bionts; 2. The phenomena of of the duplication of parts; 3. All forms of organic reproduction. Let us briefly consider these categories.

1. *Regeneration.*—All living organisms have, to a greater or less degree, the ability to repair injuries; indeed, we must regard the power of regeneration as coextensive with life, but

the capacity varies enormously in the different species. In man the power is very small, though more extensive than is generally realized. Among Annelids are species, the individuals of which may be divided in two, and each piece can regenerate all that is needed to render it a complete worm. We sometimes see a small fragment of a plant, a single switch of a willow, for instance, regenerate an entire tree, roots, trunk, branches, leaves, flowers, and all. In the last instance a few cells possess a latent formative force, which we recognize by its effects, but cannot explain. We perceive, therefore, that each individual has, as it were, a scheme or plan of its organization to which it strives to conform. As long as it actually does so, the cells perform their routine functions; but when an injury destroys or removes some portion, then the remaining cells strive to conform again to the complete scheme, and to add the missing fragment. The act of regeneration of lost parts strikes the imagination almost as an intelligent pursuit by the tissues of an ideal purpose.

Our knowledge of the regeneration power has recently received important extensions through the noteworthy experiments of Nussbaum[3] and Gruber,[4] who have demonstrated, independently, the possibility of dividing unicellular animals so that each piece will regenerate the missing parts. In this manner the number of individuals can be artifically multiplied. For example: Nussbaum divided a well-isolated Oxytricha into two equal parts, either transversely or longitudinally, and found that the edges of the cut became soon surrounded with new cilia. Although some of the substance of the body, or even a nucleus, was lost through the operation yet, by the following day, the two parts converted themselves into complete animals with four nuclei and nucleoli (*Nebenkerne*) and the characteristic ciliary apparatus. "The head piece has formed a new hind end; the right half, a new left half." The

[3] M. Nussbaum, *Ueber spontane und kunstliche Zellteilung*, Sitzungsb. d. neiderrh. Ges. f. Nat. u. Heilkunde, Bonn, 15, Dez., 1884.

[4] A. Gruber, *Ueber kunstliche Teilung bei Infusorien*, Biol. Centralblatt, Bd. IV, No. 23, 717–722.

newformed duplicate Infusoria multiplied subsequently by spontaneous division. From one Oxytrachia cut in two, Nussbaum succeeded in raising ten normal animalcules, which subsequently all encysted. After an unequal division, the parts are both still capable of regeneration, but parts without a nucleus did not survive, which suggests that the formative energy is in some way bound up with the nucleus. But nucleate pieces may break down. Thus, all attempts at artificial multiplication of the multinucleate Opalina failed, although the division of Actinosphærium had been successfully made by Eichhorn as long ago as in the last century. Pelomyxa palustris has been successfully divided by Greef, and Myastrum radians by Haeckel.

Gruber (*l. c.*, p. 718) describes his experiments with Stentor: "If one divides a Stentor transversely through the middle, and isolates the two parts, one finds on the cut surface of the hind part, after about twelve hours, a complete peristomial field with the large cilia and buccal spiral newly formed. On the other hand, the piece on which the old mouth is situated has elongated itself backwards, and attached itself in the manner peculiar to these Infusoria. If one has made a longitudinal section, so that the peristom is cut in two, then the peristoms both complete themselves and the lateral wounds heal over. I have repeatedly separated, by transection, pieces considerably less than half of the original Stentor, and these have also regenerated themselves to complete animals." Gruber, too, observed that artificially divided Infusoria were capable of subsequent spontaneous multiplication. If the section is not very deep, there may arise double monsters; but here, just as in spontaneous divisions, as long as there remains an organic connecting band, the two parts act as one individual, showing that the nervous actions are not restricted to determined paths. Gruber also adds that two divided pieces may be reunited if brought together quickly enough. The observation thus briefly announced is of such extreme interest and importance that the publication of the full details of the experiment will be eagerly awaited. Gruber adds that at present we can-

not go much beyond the proof of existence, to a high degree, of the regenerative capacity in unicellular organisms, He also makes the significant observation that in the Protozoa, we have to do foremost with changes of function ; in the Metazoa, with growth also.

2. *Duplication of parts.*—In these anomalies we find an organ which, although an extra member, yet still conforms to the type of the species. For example : a frog is found with three posterior limbs ; dissection proves the third leg to agree anatomically with the typical organization of the frog's hind leg. In determining the importance to be attributed to this evidence, it should be remembered, on the one hand, that these instances are by no means unusual ; on the other, that the agreement with the normal structure is not uniform.

3. *Asexual reproduction.*—When a species multiplies by fission of any kind, we must assume that each part, after division, possesses the formative tendency, since we see it build up what is necessary so complete the typical organization of the individual. Again : a bud of a hydroid or polyzoon, although comprising only a small part of the body, is equally endowed with this uncomprehended faculty. In pseudova we reach the extreme limit ; in aphis, for example, the parent gives off a single cell, the capacity of which, to produce a perfect and complicated individual, fully equals the like capacity of a hydroid bud or of half a worm.

The evidence forces us to the conclusion that the formative force or cause is not merely the original disposition of the forces and substances of the ovum, but that *to each portion of the organism is given : 1. The pattern of the whole organism ; 2. The partial or complete power to reproduce the pattern.* The italicized formula is, of course, a very crude scientific statement, but it is the best which has occurred to me. The formative force, then, is a diffused tendency. The very vagueness of the expression serves to emphasize our ignorance concerning the real nature of the force. In this connection, I venture to insist upon the fact that we know little or nothing concerning any of the fundamental properties of life, because I think the

lesson of our ignorance has not been learned by biologists. We encounter, not infrequently, the assertion that life is nothing but a series of physical phenomena ; or, on the other hand, what is less fashionable science just now, that life is due to a special vital force. Such assertions are thoroughly unscientific ; most of them are entirely, the remainder nearly worthless. Of what seems to me the prerequisites to be fulfilled before a general theory of life is advanced, I have written elsewhere.[5]

II. Conception of Death.

My thesis reads : There are two forms of death. These are *first*, the death of the single cells ; *second*, the death of multicellular organisms. Death in the one case is not homologous with death in the other.

Weismann assumed the complete homology of the two forms of death. Without this assumption, his hypothesis of the immortality of unicellular organisms falls to the ground and with it falls the entire superstructure of his speculations upon germ plasm. Oscar Hertwig (*Zeit und Streitfragen, Heft* 1) has already expounded, very clearly, the dependence of the theory of germ plasm upon the hypothesis of unicellular immortality ; it would, therefore, be superfluous to discuss it here.

The conception of the biological problem of death, to which I still hold, was formed several years before Weismann's first publication, which appeared in 1882, with the title, " *Ueber die Dauer des Lebens.*" He has further defended his view in his article, "*Ueber Leben und Tod*" (1884), and has steadfastly adhered to it since. In the years 1877–1879 I published my theoretical interpretation of the problem.[6] This interpretation became the starting point of elaborate special investigations, by which I endeavored to advance the solution of the problem and, in fact, observation and experiment have confirmed the

[5] C. S. Minot, *On the conditions to be filled by a theory of life*, Proc Amer. Assoc. Adv. Sc., XXVIII, 411.

[6] Proc. Boston Soc. Nat. Hist., XIX, 167 ; XX, 190.

original thesis.[7] Moreover, in an especial short article I have directed attention to the fact that Weismann has not considered the essential issue of the problem. The difficulties pointed out still remain, and, according to my conviction, cannot be removed. Weismann passes these difficulties by and carries out his speculations without first securing a basis for them. His method is illustrated by the following quotation: "I have, perhaps, not to regret that I cannot here discuss the article referred to (Minot's Article in *Science*, Vol. IV, p. 398); nevertheless, almost all objections which are there made to my views are answered in the present paper." (Weismann, Zur Frage nach der Unsterblichkeit der Einzelligen, Biol. Centralbl., IV, 690, Nachschrift). I have studied the paper with conscientious care and cannot admit that the objections have been answered. On the contrary, I maintain now, as formerly, the judgment: "He misses the real problem." For this reason I hold it to be unnecessary to discuss the details of Weisman's exposition, because—if I am right—he has not considered the actual problem of death at all. "He misses the real problem." The following reasoning leads to this decision: Protozoa and Metazoa consist of successive generations of cells; in the former the cells separate; in the latter they remain united; the death of a Protozoa is the annihilation of a cell, but the death of a Metazoon is the dissolution of the union of cells. Such a dissolution is the result of time, that is to say, of the period necessary to the natural duration of life, and we call it, therefore, "*natural death.*" Moreover, we know that natural death is brought about by gradual changes in the cells until, at last, certain cells, which are essential to the preservation of the whole, cease their functions. Death, therefore, is a consequence of changes which progress slowly through successive generations of cells. These changes cause senescence, the end of which is given by death. If we wish to know whether death, in the sense of natural death, properly so called, occurs in Protozoa or not, we must first pos-

[7] Journal of Physiology, XII, and Proc. A. A. A. S., XXXIX, (1890).

sess some mark or sign, by which we can determine the occurrence or absence of senescence in unicellular organisms.

Around this point the whole discussion revolves. Certainly a simpler and more certain conclusion could hardly be drawn than that the death of a Metazoon is not identical, *i. e.*, homologous with the death of a single cell. Weismann tacitly assumed precisely this homology, and bases his whole argument on it. In all his writings upon this subject, he regards the death of a Protozoon as immediately comparable with the death of a Metazoon. If we seek from Weismann for the foundation of this view we shall have only our labor for our pains. Starting from this view Weismann comes to the strictly logical conclusion that the Protozoa are immortal. This is a paradox! In fact, if one compares death in the two cases, from Weismann's standpoint, then we must assume a difference in the causes of death, and conclude that the cause in the case of the Protozoa is external only, while in the Metazoa it is internal only, for, of course, we may leave out of account the accidental deaths of Metazoa. If we approach the problem from this side, we encounter the following principal question: Does death from inner causes occur in Protozoa? Weismann gives a negative answer to this question, with his assertion that unicellular organisms are immortal. The assertion remains, but the proof of the assertion is lacking. In order to justify the assertion, it must be demonstrated that there does not occur in Protozoa a true senescence, showing itself gradually through successive generations of cells. Has Weismann furnished this demonstration? Certainly not. He has, strictly speaking, not discussed the subject. It is clear that we must first determine whether natural death from senesence occurs in Protozoa or not, before we can pass to a scientific discussion of the asserted immortality of unicellular beings. The problem cannot be otherwise apprehended. Weismann has not thus conceived it, therefore the judgment stands against him: *he misses the real problem.*

Senesence has been hitherto little investigated; for many years I have been studying it experimentally and have tried

to determine its exact course. My paper, "Senesence and Rejuvenation," affords evidence of new facts proven by these experiments. I believe I have thus won the right to oppose my view to the pure speculations of Weismann.

(*To be continued.*)

LOST CHARACTERISTICS.

By Alpheus Hyatt.

Dr. Minot having noticed, in the translation of his article "On Heredity and Rejuvenation," an accidental omission of quotation of work done by paleontologists on the loss of characteristics in the development of animals, has most courteously asked me to follow his essay by an article dealing with this question. I gladly avail myself of this opportunity on account of the advantages offered where similar subjects can be consecutively treated from different points of view, and because Dr. Minot's article, on account of his great and deserved reputation in embryology, will reach the students of existing biological phenomena, and perhaps induce some of them to read a connected publication.

The loss of characteristics is not so readily observed by a student of the biology of existing animals or neobiologist, as by the paleobiologist or student of fossils, because the latter necessarily deals with series of forms often persisting through long periods of time, and is led, especially if he follow more recent methods of research, to study these in great detail. The observer of these remains is not, as is falsely imagined, limited to fragments, but can and does work out of the hard matrix the external skeletons or shells even of embryos, and can, in the corals, brachiopoda, mollusca, echinodermata and even in protozoa, follow the entire life history of these parts in the individual. He has also the further advantage of availing himself of the knowledge amassed by the neobiologist and neoembryologist, the works of Cope, Beecher, Schuchert, Gurley,

Jackson and others, written in the last thirty years in this country and in Europe. The new school of Paleobiology also insists upon the close study of series of forms and rejects the methods usually pursued by the neoembryologist, who, as a rule, selects his objects of study and pursues his comparisons upon the old basis of comparative anatomy and with but little regard to the serial connections of forms. The importance of studying the seriality in structure of the members of the same group, those gradations, which lead from one variety to another, one species to another, one genus to another, until they may end in highly differentiated and often degraded offshoots, with as strange and unique developments as they have adult characters, seems not, as yet, to have attracted the attention of the students of development among recent animals as it has that of paleobiologists. The prevalent modes of study of living types has consequently led to noticing the phenomena of omission of hereditary characters only in an isolated way, and from the time of Balfour's "Comparative Embryology" these omissions occurring in the embryo have been named abbreviations, shortenings and omissions of development, and various attempts have been made to explain them upon more or less general grounds of inference. Prof. Cope and the writer and some other authors have been for a number of years publishing observations upon this class of phenomena under the title of the law of acceleration, asserting that in following out the history of series in time, or of existing series in structure, there was observable a constant tendency in the successive members (species, genera, etc.) of the same natural group to inherit the characters of their ancestors at earlier stages than those in which they appeared in these ancestors. That as a corollary of this tendency, the terminal forms eventually skipped or omitted certain ancestral characteristics, which were present in the young of the preceding or normal forms of the same series. and also in the adult stages of development of more remote ancestors of the same genetic stock or series. This law has since been independently rediscovered by several other naturalists, notably Würtemburger in Germany, and Buckman in England. The writer has lately christened this as the law of

Tachygenesis[1] in allusion to the general character of the phenomena.

In a late paper,[2] the writer reviewed Prof. Cope's and Haeckel's views of this law, and contrasted them with his own, and it seems advisable to give these remarks again in this connection.

Professor Cope has given the fullest explanation of this law, but has joined it with retardation. Thus, from his point of view, if I rightly understand him, inexact parallelism in development or failure to reproduce any hereditary characteristics is due to a tendency which appears in organisms and works in parallel lines with acceleration, the law being in his conception of a double nature. Thus he says, on page 142 of his "Origin of the Fittest," "The acceleration in the assumption of a character progressing more rapidly than the same in another character, must soon produce, in a type whose stages were once the exact parallel of a permanent lower form, the condition of inexact parallelism. As all the more comprehensive groups present this relation to each other, we are compelled to believe that acceleration has been the principle of their successive evolution during the long ages of geologic time. Each type has, however, its day of supremacy and perfection of organism, and a retrogression in these respects has succeeded. This has, no doubt, followed a law the reverse of acceleration, which has been called retardation. By the increasing slowness of the growth of the individuals of a genus, and later and later assumption of the characters of the latter, they would be successively lost. To what power shall we ascribe this acceleration by which the first beginnings of structure have accumulated to themselves through the long geologic ages, complication and power, till from the germ that was scarcely born into a sand lance, a human being climbed the complete scale, and stood easily the chief of the whole." And again, on page 182 of the same work : "Acceleration signifies addition to the number of those repetitions during the period

[1] "Phylogeny of an Acquired Characteristic." Proc. Am. Phil. Soc. Philadelphia, XXXII, No. 143.

[2] "Bioplastology and the Related Branches of Biologic Research." Proc. Bost. Soc. Nat. Hist., XXVI, p. 77–81.

preceding maturity, as compared with the preceding generation, and retardation signifies a reduction of the numbers of such repetitions during the same time." Thus, from Cope's point of view, tachygenesis is the law of progression, and retardation is the law of retrogression, and they are both essential parts of his law of acceleration and retardation.

Haeckel alludes in general terms to the law of abbreviated development in his "Morphologie der organismen," and in his "Anthropogenie," published in 1874, substantially agrees with Cope in his view of the law and uses the term "palingenesis" for the exact repetition of characteristics which occurs in the earlier and simpler forms of a phylum and "coenogenesis" for the abbreviated or highly accelerated cases of inexact parellism of the young of more complex forms with their ancestors. There is, however, an objection to this mode of using the last term which I mentioned also in writing the paper quoted.[3]

[3] During the writing of this paper I took from Cope the statement made above, although unable to find any verification of it in Haeckel's Anthropogenie (1st and 2d editions both dated 1874), but, since the above was in press, I obtained a copy of the 4th edition (1891) and the reading of this has caused me to entirely alter my opinion with regard to Haeckel's opinions. He certainly had at that time, 1891, what seems to me erroneous and inadequate view of the nature and action of the laws of tachygenesis and gave it too limited application. He also used the terms, palingenesis and cenogenesis differently from the way in which Cope and others have used them in this country.

Haeckel states (Anthropogenie, 4th edition, Leipzig, p. 9, 1891) that "Palingenetische Processe oder keimesgeschichtliche Wiederholungen nennen wir alle jene Erscheinungen in der individuellen Entwickelungsgeschichte, welche durch die conservative Vererbung getreu von Generation zu Generation übertragen worden sind und welche demnach einen unmittelbared Rückschluss auf entsprechende Vorgänge in der stammesgeschichte der entwickelten Vorfahren gestatten. Cenogenetische Processe hingegen oder keimesgeschichtlIche *Störungen* nennen wir alle jene vorgänge in der keimesgeschichte, *whelche* nicht auf solche vererbung von uralten stammformen zurüchführbar, vielmehr *erst spater durch Anpassung der Keime oder der Jugendform* an bestimme Bedingungen der Keimesentwickelung hinzugekomen sind. Diese ontogenetischen Erscheinungen *sind fremde zuthaten* welche *durchaus keinen unmittelbaren Schluss anf entsprechende Vorgange in der stammesgeschichte* der Ahnenreihe erlauben, vielmehr die Erkenntniss der letzteren geradezu fälschen und verdecken."

So far as one can get at Haeckel's oplnions from such expressions as the above it is obvious that he views shortened or abbreviated development in a very distinct light from that to which I am accustomed. He speaks of it as due to the introduction of "fremde zuthaten" as "Cenogenetische oder Störungsgeschichte"

and further to make his meaning clearer, on page 11 he divides cenogenetic phenomena into "Ortsverschiebungen oder Heterotopien," and, on page 12, "Zeitverschiebungen oder Heterochronien." Organs or parts may be developed heterotopically, that is, out of place or in a different part of the body from that in which they originated in the ancestors; or heterochronically, that is earlier in time during the life of the individual than that in which they originated, and he also speaks of the latter as "ontogenetische Acceleration," using exactly the adjective applied in this country many years beforehand, but that fact does not seem to have been considered worthy of his attention. Haeckel then proceeds to add: "Das umgekehrte gilt von der verspäteten Ausbildung des Darmcanals, der Leibeshöhle, der Geschlechtsorgane. Hier liegt offenbar eine Verzögerung oder Verspätung, eine *ontogenetische Retardation.*" This is probably what Cope alludes to in his quotation of Haeckel, and certainly this is a restatement of Cope's law of retardation with, however, the ommission of any reference to the original discoverer. It will be gathered from the text above that I view acceleration firstly, as a normal mode of action or tendency of heredity acting upon all characters that are genetic, or, in other words, derived from ancestral sources; secondly, that a ctetic, or, in other words, a newly acquired character must become genetic before it becomes subject to the law of tachygenesis. Haeckel has evidently confused ctetic characters like those of the so called ovum of Taenia, the Pluteus of Echinoderms and the grub, maggot, caterpillars of insects, which have caused the young to deviate more or less from the normal line of development, as determined by the more generalized development of allied types of the same divisions of the animal kingdom, with the normal characters that are inherited at an early stage in the ontogeny and considers them all as heterochronic. It is very obvious that they are quite distinct and that, while the ctetic characters may have been larval or even possibly embryonic in origin, and may not have affected perceptibly the adult stage at any time in the phylogeny of the group, they are, nevertheless, subject to the law of acceleration and do affect the earliest stages as has been shown in Hyatt's and Arm's book on Insecta. Such characteristics do, of course, contradict the record, if we consider that the record ought have been made by nature according to anthropomorphic standards, and in such misleading phraseology they are falsifications of the ontogenetic recapitulation of the phylogeny. In a proper nomenclature, framed with due regard to natural standards, such expressions are inadmissable. There is absolutely no evidence that characteristics repeated in the younger stages of successive species and types owe their likeness to ancestral characters to other causes than heredity. This likeness may be interfered with or temporarily destroyed by extraordinary changes of habit, as among the larvae of some insecta and the forms alluded to above, or among parasites in different degrees, but the obvious gradations of structures in many of these series show that hereditary tendencies are not easily changed in this way. There are comparatively very few forms having doubtful affinities even among the parasites. It is also evident that the novel larval characters originating in the young in their turn speedily become hereditary and are incorporated in the phylogeny and recapitulated in the ontogeny.

It may be seen from this that in dividing tachygenesis into palingenesis and cenogenesis the writer has followed Cope rather than Haeckel, and there is a seri-

Either through want of acquaintance with good examples of retardation or because of a different point of view, I have not been able to see any duplex action in the law of acceleration. To me it is the same law of quicker inheritance which is acting all the time in the phylum at the beginning, middle, and end of its history, as will be seen by the explanation given above. In Insecta[4] I have tried to apply it to the explanation of the peculiar larval forms of those animals which often present retrogression through suppression of ancestral characters in the young, although their adults are perfectly normal and perhaps progressive. Consequently, palingenesis and coengenesis are, from my point of view, simply different forms of tachygenesis, and there is no boundary or distinction between them. In other words, retardation or retrogression occurs because of the direct action of tachygenesis upon more suitable and more recently acquired characteristics which are driven back upon and may directly replace certain of the ancestral characters causing them to disappear from ontogenetic development.[5]

ous objection to the use of cenogenesis at all, since it is from Κενός meaning strange, and was first applied by Haeckel in such a way that both by his statements, and the derivation, it ought to be confined to types like larvae of the Echinodemata Insect, etc., and parasites in which acquired characters do interfere with the ontogenic recapitulation for a certain time. Normal types, in which tachygenesis occur in a marked way might be called tachygenetic. Palingenesis and palingenetic might be confined to generalized forms in which the ontogeny was a more or less prolonged recapitulation of the phylogeny, and coenogenesis would thus be properly confined to its original field wherever ctetic characters were introduced. This would avoid the need of using a new term.

[4] Guides for Science Teaching, Boston Soc. Nat. Hist., No. 8.

[5] Specialization by reduction of parts is evidently included under the head of retardation by Cope; thus in Origin of the Fittest, p. 353, he says that "change of structure during growth is accomplished either by addition or parts (acceleration) or by subtraction of parts (retardation)." So far as my experience goes in the major number of cases, the parts of characters that are undergoing reduction disappear according to the law of tachygenesis. They reappear in the ontogeny at earlier and earlier stages, or exhibit this tendency in the same way as characters of the progressive class, but their development is not so complete as in ancestral forms. In this sense they can be regarded as retarded or thrown back in their development. There is, however, another way of formulating the expression for this. Instead of regarding this disappearance by retrogressive gradations as due to a tendency opposed to acceleration, is it not a tendency of the same

The law of tachygenesis as defined by the writer acts upon all characteristics and tendencies alike, and is manifested in genetically connected phyla by an increasing tendency to con-concentrate the characteristics of lower, simpler, or earlier occurring, genetically connected forms in the younger stages of the higher, more complicated or more specialized, or more degraded, or later occurring forms of every grade, whether the characteristics arise in adults or in the younger stages of growth. Since my first publication in 1866, the law has become clearer to me, but I have made no fundamental change in the conception. The application of the law to degenerative characteristics appears to me to explain why there are degenerative forms in the phylum which are indicated by the senile stages of the individual.

The degenerative changes of the senile period may, and practically in all cases do, tend to the loss of characteristics of the adult period and consequently in extreme cases bring about not only the loss of a large proportion of progressive characteristics, but loss in actual bulk of the body as compared with adults, as has been stated above. This is usually regarded as due to the failure of the digestive organs or defective nutrition, and this may be true in many examples; but, on the other hand, it often begins in individuals long before there is any perceptible diminution in size, and may occur in dwarfs and in some degenerate species in the early stages, and finally in series of species according to the law of tachygenesis, so that

kind? That is to say, do not the parts and characters show a tendency to disappear earlier and earlier, and are they not, in most cases, at the time of disappearance, present also in earlier stages of growth than that in which they originated in ancestral forms?

Is not the case of the wisdom teeth exceptional? The frequently extremely late external appearance of these is not accompanied by a later origin of their rudiments in the jaw. Although they may not appear in many cases above the gum until a person is past fifty, is not this retardation in becoming externally visible due primarily to the fact that they are deficient in growth power (tending to disappear from disuse, etc.), and secondarily to their internal position. When they cease to be able to break through the gum, will they not still continue to develop at the same stage as the other teeth, and will not their rudiments be likely tobe present at this early stage long after they have ceased developing into perfect teeth?

one is led to believe that the tendency to the earlier inheritence of degenerative modifications producing retrogression is inheritable like the tendency to the earlier inheritance of additional or novel characteristics producing progression. Thus, this law applied to progressive or retrogressive groups explains the mode in which their progression or retrogression is accomplished so far as the action of the laws of genesiology (science of heredity) are concerned.

In the same essay on Bioplastology, the writer reviewed Dr. Minot's law of growth, and in this and in his Phylogeny, quoted above, used it to throw light upon one of the most difficult problems of evolution.

It is a general law of unique importance, as readily observable in the growth of skeletons and shells of all kinds, and therefore as obvious in fossils as in the famous guinea pigs studied by Dr. Minot. This law enabled the writer to get what seemed to him a clearer view of the action of tachygenesis. See Bioplastology (p. 76).

Minot's researches enable one to see clearly that the reduction of parts or characteristics which takes place through the action of the law known as the law of acceleration in development (often also descriptively mentioned as abbreviated or concentrated development) cannot be considered as due to growth.

"It seems probable from my own researches published in various communications, but more especially in the 'Genesis of the Arietidae,'[6] that the action in this case is *a mechanical replacement of the earlier and less useful ancestral characteristics and even parts by those that have arisen later in the history of the group.* We can fully understand the phenomena of acceleration in development only when we begin by assuming that the characteristics last introduced in the history of any type were more suitable to the new conditions of life on the horizon of occurrence of the species than those which characterized the same stock when living on preceding horizons or in less specialized habitats. These new characters would necessarily, on

[6] Smithsonian Contributions to Knowledge, v. 26, p. 40–48, 1889; also, Mus. Comp. Zool., v. 16, 1889.

account of their greater usefulness and superior adaptability, ultimately interfere with the development of the less useful ancestral stages and thus tend to replace them. The necessary corollary of this process would be tachygenesis or earlier appearance of the ancestral stages in direct proportion to the number of new characteristics successively introduced into the direct line of modification during the evolution of a group.

If this be true, it can hardly be assumed that the loss of characteristics and parts taking place in this way is directly due to growth force. If growth has anything to do with these phenomena, it must act indirectly, and, as in the repetition of other similarities and parallelisms, under the controlling guidance of heredity.

VARIATION AFTER BIRTH.

By L. H. Bailey.

At the present time, our attention is directed to differences or variations which are born with the individual. We are told that variation which is useful to the species is congenital, or born of the union—or the amalgamation in varying degrees—of parents which are unlike each other. From the variations which thus arise, natural selection chooses those which fit the conditions of life and destroys the remainder. That is, individuals are born unlike and unequal, and adaptation to environment is wholly the result of subsequent selection.

These are some of the practical conclusions of the NeoDarwinian philosophy. It seems to me that we are in danger of letting our speculations run away with us. Our philosophy should be tested now and then by direct observation and experiment, and thus be kept within the limits of probability. The writings of Darwin impress me in this quality more than in any other,—in the persistency and single-mindedness with which the author always goes to nature for his facts.

In this spirit, let us drop our speculations for a moment, and look at some of the commonest phenomena of plant life as they transpire all about us. We shall find that, for all we can see, most plants start equal, but eventually become unequal. It is undoubtedly true that every plant has individuality from the first, that is, that it differs in some minute degree from all other plants, the same as all animals possess differences of personality; but these inital individual differences are often entirely inadequate to account for the wide divergence which may occur between the members of any brood before they reach their maturity.

The greater number of plants, as I have said, start practically equal, but they soon become widely unlike. Now, everyone knows that these final unlikenesses are direct adaptations to the circumstances in which the plant lives. It is the effort to adapt itself to circumstances which gives rise to the variation. The whole structure of agriculture is built upon this fact. All the value of tillage, fertilizing and pruning lies in the modification which the plant is made to undergo. Observe, if you will, the wheat fields of any harvest time. Some fields are "uneven," as the farmers say; and you observe that this unevenness is plainly associated with the condition of the land. On dry knolls, the straw is short and the plant early; on moister and looser lands, the plant is tall, later, with long, well-filled heads; on very rich spots, the plants have had too much nitrogen and they grow too tall and "sappy," and the wheat "lodges" and does not fill. That is, the plants started equal, but they ended unequal. Another field of wheat may be very uniform throughout; it is said to be "a good stand," which only means, as you can observe for yourself, that the soil is uniform in quality and was equally well prepared in all parts. That is, the plants started equal, and they remained equal because the conditions were equal. Every crop that was ever grown in the soil enforces the same lessons. We know that variations in plants are very largely due to diverse conditions which arise after birth.

All these variations in land and other physical conditions are present in varying degrees in wild nature, and we know

that the same kind of adaptations to conditions are proceeding everywhere before our eyes. We cannot stroll afield without seeing it. Dandelions in the hollows, on the hillocks, in the roadside gravel, in the garden—they are all different dandelions, and we know that any one would have become the other if it had grown where the other does.

But aside from the differences arising directly from physical conditions of soil and temperature and moisture, and the like, there are differences in plants which are forced upon them by the struggle for life. We are apt to think that, as plants grow and crowd each other, the weaker ones die outright, because they were endowed with—that is, born with—different capabilities of withstanding the scuffle. As a matter of fact, however, the number of individuals in any area may remain the same or even increase, whilst, at the same time, every one of them is growing bigger. Early last summer I staked off an area of twenty inches square in a rich and weedy bit of land. When the first observations were made on the the 10th of July, the little plat had a population of 82 plants belonging to 10 species. Each plant was ambitious to fill the entire space, and yet it must compete with 81 other equally ambitious individuals. Yet, a month later, the number of plants had increased to 86, and late in September, when some of the plants had completed their growth and had died, there was still a population of 66. The censuses at the three dates were as follows:

	July 10.	Aug. 13.	Sept. 25.
Crab grass (Panicum sanguinale)	22	20	15
Black Medick (Medicago lupulina)	16	17	15
Purslane	14	15	12
White Clover	12	13	8
Red Clover	9	11	8
Red-root (Amarantus retroflexus)	4	4	4
Ragweed (Ambrosia artemisæfolia)	2	2	2
Pigeon-grass (Setaria glauca)	1	2	3
Pigweed (Chenopodium album)	1	1	0
Shepherd's Purse	1	1	1
	82	86	66

What a happy family this was! In all this jostle up to the middle of August, during which every plant had increased its

bulk from two to twenty times, only the crab grass—apparently the most tenacious of them all—had fallen off; and yet the area seemed to be full in the beginning! How then, if all had grown bigger, could there have been an increase in numbers, or even a maintenance of the original population? In two ways: first, the plants were of widely different species of unlike habits, so that one plant could grow in a place where its neighbor could not. Whilst the pigweed was growing tall, the medick was creeping beneath it. This is the law of divergence of character, so well formulated by Darwin. It is a principle of wide application in agriculture. The farmer "seeds" his wheat-field to clover when it is so full of wheat that no more wheat can grow there, he grows pumpkins in a cornfield which is full of corn, and he grows docks and sticktights in the thickest orchards. Plants have no doubt adapted themselves directly, in the battle of life, to each other's company.

The second and chief reason for the maintenance of this dense population, was the fact that each plant grew to a different shape and stature, and each one acquired a different longevity; that is, they had varied, because they had to vary in order to live. So that, whilst all seemed to have an equal chance early in July, there were in August two great branching red-roots, one lusty ragweed and 83 other plants of various degrees of littleness. The third census, taken September 25th, is very interesting, because it shows that some of the plants of each of the dominant species had died or matured, whilst others were still growing. That is, the plants which were forced to remain small also matured early and thereby, by virtue of their smallness, they had lessened, by several days, the risk of living, and they had thus gained some advantage over their larger and stronger companions, which were still in danger of being killed by frost or accident. When winter finally set in, the little plat seemed to have been inhabited only by three big red-roots and two small ones and by one ragweed. The remains of these six plants stood stiff and assertive in the winds; but if one looked closer he saw the remains of many lesser plants, each "yielding seed after his kind," each one, no

doubt, having impressed something of its stature and form upon its seeds for resurrection of similar qualities in the following year. All this variation must have been the result of struggle for existence, for it is not conceivable that in less than two square feet of soil there could have been other conditions sufficiently diverse to have caused such marked unlikenesses; and I shall allow the plat to remain without defilement that I may observe the conflict in the years to come, and I shall also sow seeds from some of the unlike plants. From all these facts, I am bound to think that physical environment and struggle for life are both powerful causes of variation in plants which are born equal.

Still,the reader may say,like Weismann,that thesedifferences were potentially present in the germ, that there was an inherited tendency for the given red-root to grow three feet tall when 85 other plants were grown alongside of it in twenty inches square of soil. Then let us try plants which had no germ plasm, that is, cuttings from maiden wood. A lot of cuttings were taken from one petunia plant, and these cuttings were grown singly in pots in perfectly uniform prepared soil, the pots being completely glazed with shellac and the bottoms closed to prevent drainage. Then each pot was given a weighed amount of different chemical fertilizer and supplied with perfectly like weighed quantities of water. All weak or unhealthy plants were thrown out, and a most painstaking effort was made to select perfectly equal plants. But very soon they were unequal. Those fed liberally on potash were short, those given nitrogen were tall and lusty; and the variations in floriferousness and maturity were remarkable. The data of maturity and productiveness were as follows:

Phosphate of Potash.	Sulphate of Potash.	Phosphate of Soda.	Check	Phosphate of Ammonia.
68 days	99 days	65 days	67 days	104 days
23½ blooms	18 blooms	27½ blooms	26½ blooms	33 blooms

Here then, is a variation of 39 days, or over a month in the time of first bloom, and of an average of 15 flowers per plant in asexual plants from the same stock, all of which started equal and which were grown in perfectly uniform conditions, save the one element of food.

But these or similar variations in cuttings are the commonest experiences of gardeners. Whilst some philosophers are contending that all variation comes through sexual union, the gardener has proof day by day that it is not so. In fact, he does not stop to consider the difference between seedlings and sexless plants in his efforts to improve a type, for he knows by experience that he is able to modify his plants in an equal degree, whatever the origin of the plants may have been. Very many of our best domestic plants are selections from plants which are always grown from cuttings or other asexual parts. A fruitgrower asked me to inspect a new blackberry which he had raised. "What is its parentage?" I asked. "Simply a selection from an extra good plant of Snyder" he answered; that is, selection by means of suckers, not by seedlings. The variety was clearly distinct from Snyder, whereupon I named it for him. The Snyder plants were originally all equal, all divisions in fact, of one plant, but because of change of soil or some other condition, some of the plants varied, and one of them, at least, is now the parent of a new variety.

But even Mr. Weismann would agree to all this, only he would add that these variations are of no use to the next generation, because he assumes that they cannot be perpetuated. Now, there are several ways of looking at this Weismannian philosophy. In the first place, so far as plants are concerned in it, it is mere assumption, and, therefore, does not demand refutation. In the second place, there is abundant asexual variation in flowering plants, as we have seen; and most fungi, which have run into numberless forms, are sexless. In the third place, since all agree that plants are intimately adapted to the conditions in which they live, it is violence to suppose that the very adaptations which are directly produced by those conditions are without permanent effect. In the fourth place, we know as a matter of common knowledge and also of direct experiment, that acquired characters in plants often are perpetuated.

I cannot hope to prove to the Weismannians that acquired characters may be hereditary, for their definition of an acquired

character has a habit of retreating into the germ where neither they nor anyone else can find it. But this proposition is easy enough of proof, viz., plants which start to all appearances perfectly equal, may be greatly modified by the conditions in which they grow; the seedlings of these plants may show these new features in few or many generations. Most of the new varieties of garden plants, of which about a thousand are introduced in North America each year, come about in just this way. A simple experiment made in our greenhouses also shows the truth of my proposition. Peas were grown under known conditions from seeds in the same manner as the petunias were, which I have mentioned. The plants varied widely. Seeds of these plants were saved and all sown in one soil, and the characters, somewhat diminished, appeared in the offspring. Seeds were again taken, and in the third generation the acquired characters were still discernible. The full details of this and similar experiments are waiting for separate publication. The whole philosophy of "selecting the best" for seed, by means of which all domestic plants have been so greatly ameliorated, rests upon the hereditability of these characters which arise after birth ; and if the gardener did not possess this power of causing like plants to vary and then of perpetuating more or less completely the characters which he secures, he would at once quit the business because there would no longer be any reward for his efforts. Of course, the NeoDarwinians can say, upon the one hand, that all the variations which the gardener secures and keeps were potentially present in the germ, but they cannot prove it, neither can they make any gardener believe it; or, on the other hand, they can say that the new characters have somehow impressed themselves upon the germ, a proposition to which the gardener will not object because he does not care about the form of words so long as he is not disputed in the facts. Weismann admits that "climatic and other external influences" are capable of affecting the germ, or of producing "permanent variations," after they have operated "uniformly for a long period," or for more than one generation. Every annual plant dies at the end of the season, therefore whatever effect the environment may

have had upon it is lost, unless the effect is preserved in the seed; and it does not matter how many generations have lived under the given uniform environment, for the plant starts all over again, *de novo*, each year. Therefore, the environment must affect the annual plant in some one generation or not at all. It seems to me to be mere sophistry to say that in plants which start anew from seeds each year, the effect of environment is not felt until after a lapse of several generations, for if that were so the plant would simply take up life at the same place every year. This philosophy is equivalent to saying that characters which are acquired in any one generation are not hereditary until they have been transmitted at least once!

My contention then, is this: plants may start equal, either from seeds or asexual parts, but may end unequal; these inequalities or unlikenesses are largely the direct result of the conditions in which the plants grow; these unlikenesses may be transmitted either by seeds or buds. Or, to take a shorter phrase, congenital variations in plants may have received their initial impulse either in the preceding generation or in the sexual compact from which the plants sprung.

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A COMPARATIVE STUDY OF THE POINT OF ACUTE VISION IN THE VERTEBRATES.[1]

By J. R. Slonaker,
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In this preliminary sketch of a comparative study of the eyes of vertebrates, with special reference to the *fovea centralis* or point of acute vision, I shall first give the processes and methods of preparation which I have used and results obtained, and, second, the position of the *area centralis* as indicated by the retinal arteries. The microscopic descriptions and the relation of the position and shape of the eye and arrangement of the retinal elements to the habits of the animal will follow in a later paper.

[1] I wish to thank Dr. C. F. Hodge for valuable assistance and for his method of injecting the eye-ball, thus preserving it for complete sections. I am also very much indebted to Clark University for valuable aid and for apparatus and materials to further this study.

For microscopical purposes and best results it is necessary to obtain the eye fresh, at least not later than an hour after death, and subject it to the action of certain hardening liquids which will permeate and preserve without causing the retina to swell and become wrinkled. With some animals it is quite easy to preserve the retina without its becoming wrinkled or floated off (fishes, amphibians, reptiles, and some mammals), while with others (most mammals and birds) it is a more difficult task.

In order to prevent this folding and floating off of the retina, the eye is injected under pressure and immersed at the same time in a bath of hardening fluid. It is carried thus on up through the different percentages of alcohol and imbedded in celloidin.

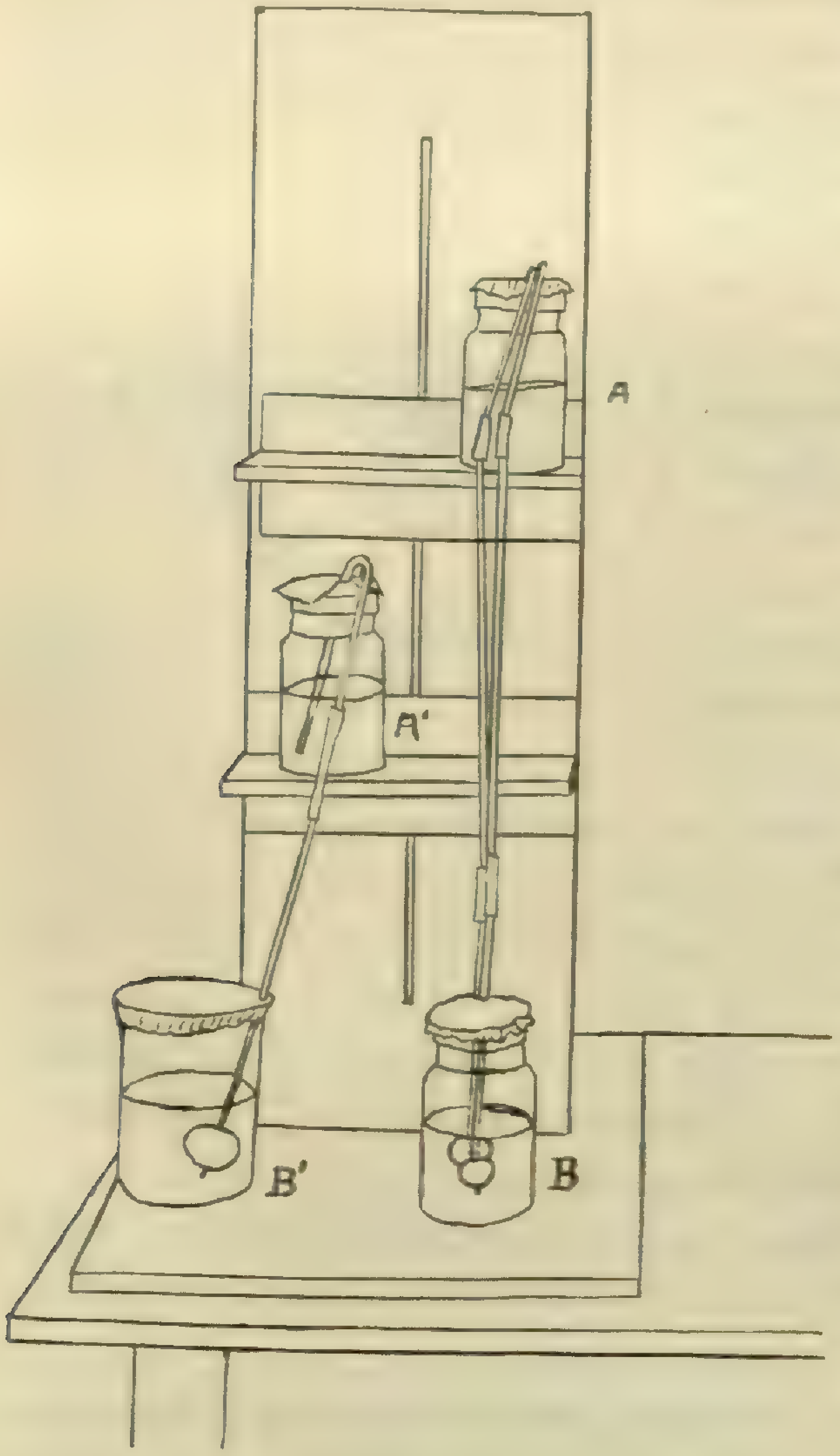

FIG. 1.

A more minute description of the method is as follows: Fig. 1 represents a rack with movable shelves, on which are placed bottles A and A′, containing the same fluid as bottles B and B′, and provided with siphons to connect with glass cannulas.

In order to insert the cannula, a hole is carefully drilled about the equator and on a meridian perpendicular to the plane in which it is desirable to obtain sections. The perforation is stretched open, rather than cut, so the sclerotic will clasp the neck of the cannula tightly. A convenient instrument for this operation is a spear-pointed dissecting needle, and not

too sharp. At the same time reach forward with the point of the needle and pierce the suspensory ligament and iris in order to open the aqueous chamber. In doing this, care is taken not to injure structures in the plane of the desired sections. A cannula of suitable size, being connected with a siphon from A or A′, is filled with the liquid and inserted. The cannula should have a fine smooth point. Great care is taken in inserting it so that the stream of fluid is not directed behind the retina to float it off. A hole is now made in the opposite side of the eye, the aqueous chamber again pierced and all aqueous and vitreous humor allowed to run out. In some animals this humor is very much more gelatinous than in others, and requires much more pressure to remove it. The hole below is then stopped with a small glass plug (Fig. 2, B), and the eye immersed in hardening fluid (Fig. 1, B). The bottles are now covered as tightly as possible with tinfoil to prevent evaporation and entrance of dust particles. The cannula and stopper should fit so tight that there is no leak. In every case the orientation of the eye is marked before it is removed from the head. This is done by sewing a small tag to the outer layers of the sclerotic (Fig. 2, C).

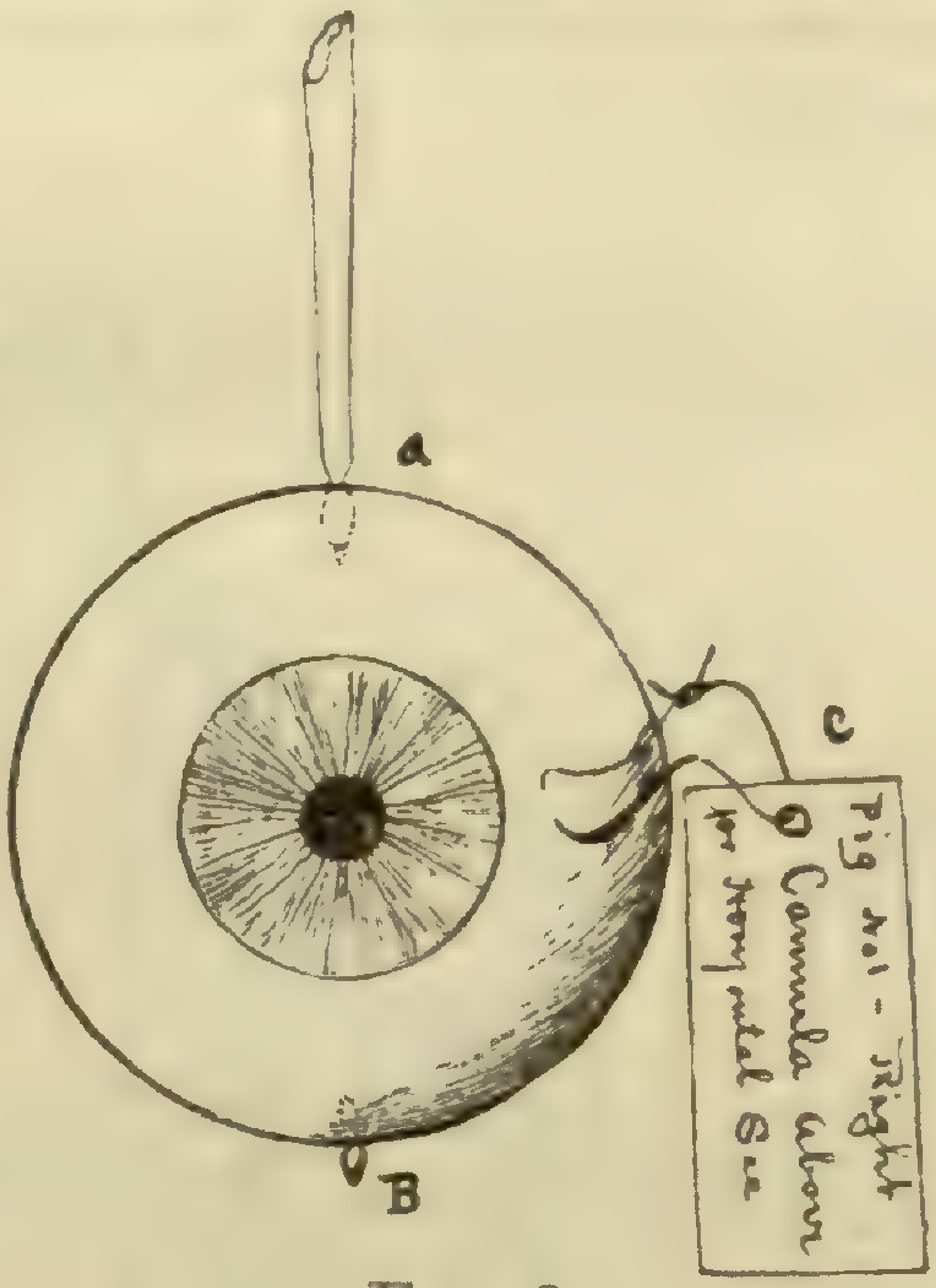

FIG. 2.

The pressure varies greatly with the kind of eye used. Those with thin walls, or containing much cartilage, birds and amphibians, require little pressure, while mammals, in general, can receive much higher. The pressures which I have found to work best vary between 28 and 36 cm.

The hardening fluid used is Perenyi's, in which the eye is allowed to remain twenty-four hours, when it is changed to 70 per cent. alcohol.

In making changes of liquids, great care should be taken that no air get into the eye, and that all the former liquid is

replaced with fresh by removing the stopper in the lower part of the eye. After remaining twenty-four hours in each of the following liquids: 80, 90, 95 per cent., absolute alcohol and absolute ether (1 part each), it is then changed to celloidin. Best results are obtained when three grades of celloidin are used—1st, very dilute; 2d, less dilute; 3d, as thick as will run. It is allowed to remain from four to six days in the first, six to eight days in the second, and ten to fifteen days in the third. If the eye is kept well under pressure throughout this process, the retina will be well preserved and lie smoothly against the choroid.

I have tried other liquids for hardening the eye whole, but with poor success. Have tried the method of Barrett and of Cuccati, but, in each case, the retina was very much wrinkled and folded, while the whole eye was much shrunken and out of shape. In vapors of osmium, I have had fairly good results with the retina, but the same trouble, due to the shrinking of the whole eye, is present. Chievitz says[2] that a fish's eye may be preserved whole, with retina lying nicely back, by simply immersing it, or even the whole head, in 80 per cent. alcohol. The hardening agent which he generally uses is 2.5 per cent. nitric acid.

Another method which I have employed with small animals, especially birds, in order to demonstrate quickly the presence or absence of a fovea, is to immerse the whole head in Perenyi's fluid for from three to five hours. This will harden the eyes so that the cornea, lens and vitreous humor may be removed, leaving the posterior half in situ. With birds I have had good results, the retina lying back smoothly so that the fovea and entrance of the nerve, marked by the pecten, may be easily seen. Fig. 3 represents diagrammatically the appearance of the retina after the front of the eye has been removed.

In order to show the angles which the lines of vision make with the median plane, sections were made through the whole head of several animals (fish, amphibians, reptiles, birds and

[2] J. H. Chievitz, Untersuchungen über die Area centralis retinae. Archiv für Anatomie und Entwickelungsgeschichte, Sup., Band, 1889, p. 141–142.

small mammals), the plane of the section passing through each fovea on the centre of the area centralis. Fig. 4 represents such a section through the foveæ *a* and *b* of a chickadee's head (*Parus atricapillus*), while the lines G H and G I show

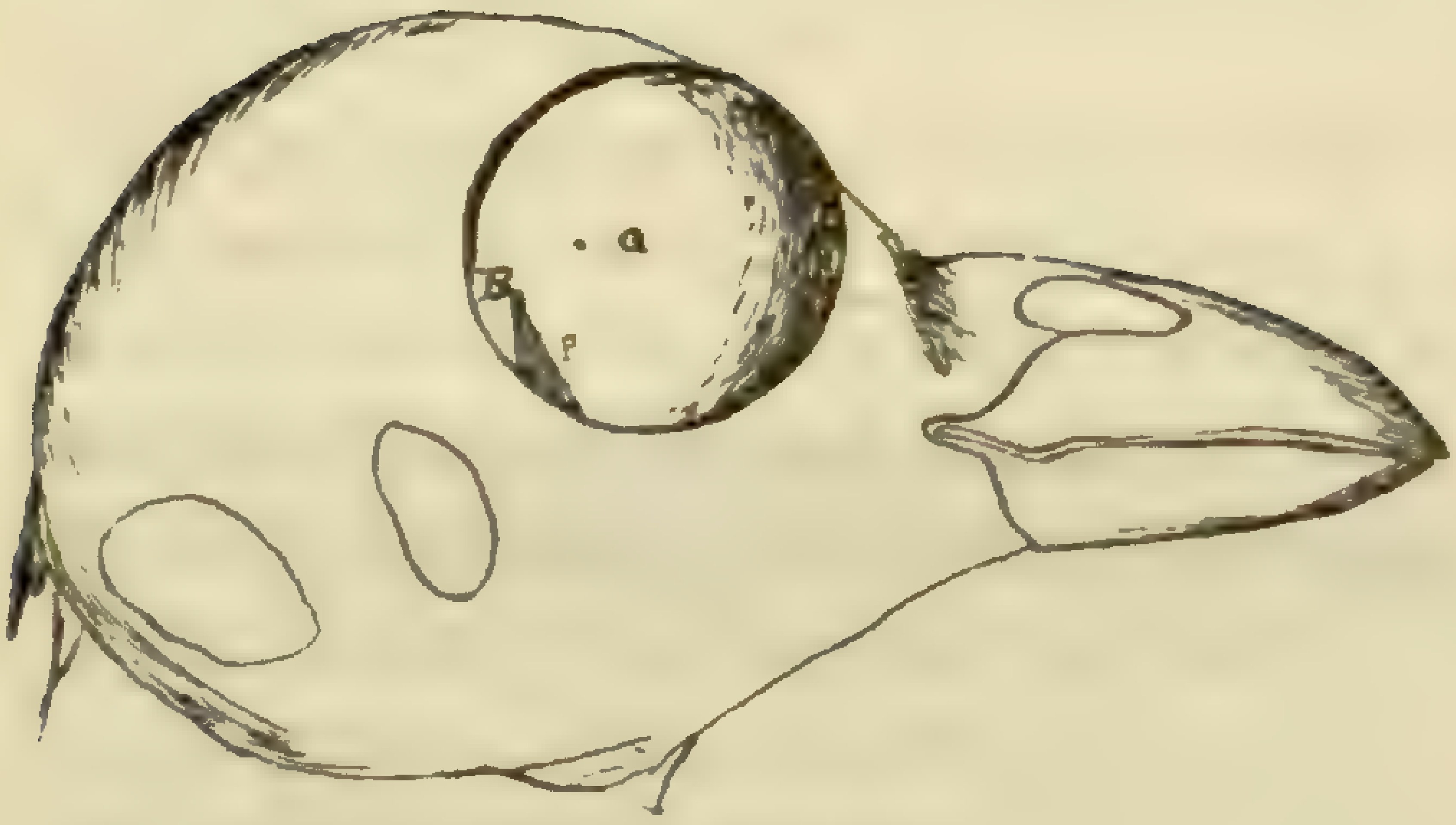

FIG. 3.

Snow-bird (*Junco hyemalis*) x 3.
A, Fovea centralis.
B, Entrance of optic nerve.
P, Pecten.

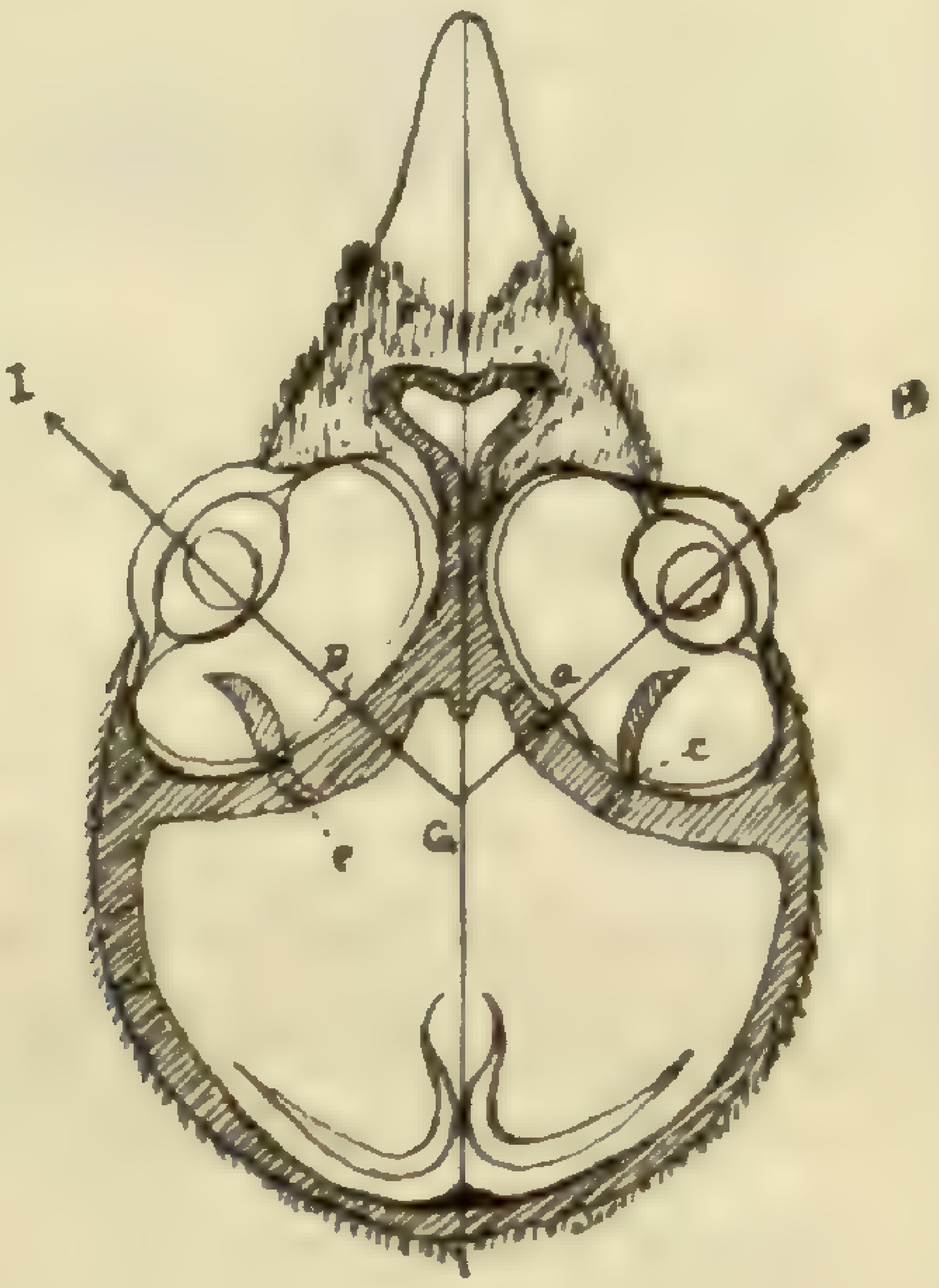

FIG. 4.

Chickadee (*Parus atricapillus*) x 3.
A and B. Foveæ.
C, C, Entrance of optic nerves.
G H and G I, Axes of vision.

the axis of vision. The dotted lines *c* mark the position of the optic nerves which enter in a plane much lower down. In order to harden the whole head, and, at the same time, decalcify the bone, it must remain longer in Perenyi's fluid (about thirty- six hours), and to preserve the cornea and lens in position, a window is made in the top of the eye that the fluids may enter.

Having had good success with simple immersion of the head, this method was tried for hardening the small eyes, and with good success. In fact, the retina proved in good condition, if not better, than when taken through by the injection method. The eye-ball, however, usually caves in when placed in 70 per cent. or 80 per cent. alcohol, but this may be prevented by simply making a small slit through the sclerotic

into the vitreous chamber before immersing in 70 per cent. alcohol to allow the liquids to pass in. Just before putting into celloidin, a window is made parallel to the plane of desired sections, and the hardened vitreous humor is easily removed without injury to the retina or other structures. This method is now used with small eyes instead of the injection, as it is so much easier of manipulation.

In order to show the relation of the retinal arteries to the area and fovea centralis, they were injected with the gelatine-carmine mass of Ranvier. In small animals this injection was made in the carotid arteries, while with large animals the eyes were removed and the injection made into that branch of the ophthalmic artery which supplies the retina. After injection, the eyes were at once cooled and hardened in alcohol. When hardened, the front half of the globe and the vitreous humor were carefully removed, exposing to view the retina, arteries, entrance of nerve, and area and fovea centralis, when present. The fovea is at once seen if it be present, but the area is sometimes very difficult to discern, and, were it not for the blood-vessels acting as land-marks, it might be overlooked altogether. Drawings were made of this posterior half, great care being taken to orient it, so that one would look into it along the axis of vision.

The results of these injections only serve to substantiate Müller's observation.[3] He states that mammals are the only class of vertebrates which possess, in the true sense, a retinal circulation, while with many mammals only a meagre circulation is present (horse and rabbit). Fish and amphibians possess a good circulation in the hyaloid membrane, while birds and many reptiles have the circulation of the pecten. Huschke states that these vessels of the hyaloid membrane and the pecten correspond to the retinal vessels in mammals. They do not, however, penetrate the retina.

With animals which have neither retinal nor hyaloid vessels, it would appear that the retina is nourished by the choroidal vessels. In fact, in animals with good retinal circulation, the capillaries do not penetrate deeper than the outer

[3] H. Müller, Anatomie und Physiologie des Auges, p. 117.

molecular layer, thus leaving the rod and cone, and outer nuclear layers without blood-vessels.[4]

Investigations show that not all vertebrates possess foveæ, but that each class has a representative which does. When there is no fovea, a well-defined area centralis is usually present. However, in some vertebrates, even an area has not been observed.

The following condensed tabulation will show the frequency of the area and fovea centralis in the eyes which have been examined.[5]

Number of different species.		No area found.	Area found.		Fovea.		
			Round.	Band-like.	Simple.	Band like or trough-like	Double.
28	Mammals	13	9	6	2		
80	Birds	1	84	32	85	23	7
9	Reptiles	1	6	2	3	2	
12	Amphibians	3	1	8	1	1	
4	Fishes	3	1		1		

From this tabulation it is readily seen, so far as experiments have gone, that in mammals the presence of a fovea is the exception while an area is the rule. The primates are the only mammals in which a fovea has been found. Most of the mammals examined have a well-defined area which is easily seen, but, in some, an area has not been demonstrated. The arrangement of the retinal vessels, however, indicates the presence of an area which is free from blood-vessels, and may correspond to the area centralis of other animals.

[4] H. Müller, Anatomie und Physiologie des Auges, p. 103.

[5] These results are partly obtained from the tabulation of J. H. Chievitz in his article: Ueber das Vorkommen der Area centralis retinae in den vier höheren Wirbelklassen. Archiv. f. Anat. u. Entwick., 1891, p. 321–325.

With birds, the presence of a fovea seems to be the rule. In fact, the domestic chicken is thus far the only exception. Many birds have a fovea and band-like area, while some have two foveæ and a band-like area connecting them.

In reptiles, the number of species provided with fovea or simple area are more nearly equal, while with amphibians and fishes, the area has frequently not been seen, and the fovea is only seldom observed.

The area centralis varies greatly in form and extent in different animals. It varies from the round form of small extent found in the cat and the weasel to the band-like form found in the horse, sheep, rabbit, frog, etc., which extends horizontally across the retina.

In the case of the fovea we also find a variety of forms and positions. In some animals it is situated on the nasal side of the entrance of the optic nerve (*fovea nasalis*), while in others it is on the temporal side (*fovea temporalis*). According to Müller,[6] in the former case we have monocular vision, while in the latter we have binocular vision. In form it varies from a mere dot-like impression, as in some lizards, to a well marked funnel-like pit in most birds, especially crow, bluejay, robin, etc., and to a trough-like depression in the crocodile which extends horizontally across the retina. Two foveæ have been found in some birds, as in swallows and terns, in which case the fovea nasalis is very near the centre of the retina, and has to do with single vision. It is also larger and deeper than the fovea temporalis, which is situated near the ora serrata and functions in double vision. According to Chievitz,[7] the tern has not only two fovæ, but a trough-like fovea connecting them, and the goose, duck and gull have a round fovea and a band-like area.

A great difference exists in the different vertebrates when their ability for acuteness of sight is considered. It varies from the most perfect sight found in man (and possibly in birds

[6] H. Müller, Ueber das Vorhandsein zweier Fovea in der Netzhaut Vieler Vogelaugen—Zehender, Klinische Monatsblätter, Sept, 1863, p. 438–440; or Anatomie und Physiologie des Auges, p. 139, 142–143.

[7] J. H. Chievitz, Ueber das Vorkommen der Area centralis retinae, Archiv. f. Anat. u. Entwick., 1891, p. 324.

also) where exceedingly fine discriminations are possible, to the limited visual power found in other animals, where only an area centralis is present. Though acute vision and a fovea have always been associated, still we cannot, at present, say that the animals which do not possess a fovea are not able to see acutely. In order to make clear the relation of sight to the habits of the animal, a much more careful observation of its visual habits, and the histological arrangement of the retinal elements will be necessary.

EDITOR'S TABLE.

—THE Antivivisectionists have been endeavoring to get a consensus of opinion on the utility of vivisection, by circulating blanks for signatures, which are attached to a few alternative opinions on the subject in point. The alternatives, excepting those expressing an unconditional affirmative and negative, were not sufficiently precise or well stated to satisfy persons of moderate views, so that it was necessary to amend them more or less to express such opinions. In the summary of the results thus obtained, the antivivisection managers omitted most of these moderate views, and only gave to the public the two extremes. The circulars were also very injudiciously distributed, as a majority of them went to persons unfamiliar with the work of scientific research, as clergymen, etc. The only persons who have a practical knowledge of the subject are original investigators in the natural sciences, physiologists and physicians. The opinions of other persons must be mostly formed at second hand.

As a body of men, those above referred to are at least as humane as any other class in the community. Their business is to relieve suffering, and they are not insensible to those of the lower animals. Naturalists, as a body, are probably more humane in their feelings towards animals than any other class in the community. Nearly all of these men are, however, well convinced not only of the propriety, but of the necessity of vivisection. It is the only method of attacking many difficult problems of physiology. It is the basis of our knowledge of the functions of the human organism, which is itself the first essential to the control of human disease and human suffering. The antivivi-

sectionists are, unwittingly, doing what they can to sustain ignorance and to prevent the relief of human suffering. They are sacrificing their fellow beings, their relatives and their friends, in preference to a few of the lower animals. Men, women and children may suffer and die; white rabbits, guinea-pigs and dogs may live. Such logic is like that of the Spanish Inquisitors, who tortured human beings under the belief that they served God and the cause of religion in so doing. There is, however, less excuse for the antivivisectionists, since knowledge is more widely distributed now than then, and the great utility of vivisection has been demonstrated over and over again.

The six national scientific societies to meet during the holidays in Philadelphia will probably express their views on this subject, and it may be confidently expected that these will accord with those of science the world over.

Intelligent people are best deceived by intelligent frauds. A fraud in order to succeed in the United States must make pretensions to superior knowledge. The alleged or actual graduate of medicine who desires to be a fraud has a pretty good field in this country; and his successes are ever with us, in spite of the opposition of the many true men of that profession. The scientific fraud has not yet developed very largely, as there is no money to be made by pretense in this direction. In fact this species of the genus is not generally a person of evil intentions, and errs chiefly through an active imagination, and perhaps sometimes through a tendency to megalomania.

We are moved to these remarks by reading an article in the December number of a Chicago Journal called Self Culture. On p. 587 we read; "Examination of the brain of such an idiot before its education has begun, shows but few brain cells, and a few nerve fibres connecting them. And when a postmortem has been made upon the child that was once an idiot but that has been lifted up by long years of patient training to citizenship in the moral and rational sphere in which we live and move, such a postmortem shows that an infinite number of brain-cells have been created *de novo*; that fibers becoming necessary have *appeared*, to connect such cells, centers of sensation and emotion and thought."

Now the author of this paragraph should refer us to the published articles which describe the removal of the brains or parts of brains of idiotic children for sectioning and microscopic investigation, and the subsequent replacement of these organs or parts of them in the crania of the children in order that they may undergo the "long years of

patient training" which follow. We would like to know the technique of the operation, and the name of the operator and that of the institution where he operates. Some grown persons might desire to secure his services, and almost everybody could point out some one else, to whom they think such a course of treatment would be useful. Some peculiar conditions might be found which it would be desirable to remove permanently, and so save the "labor of long years" etc.

The editor of the Journal on page 609 stimulates our curiosity further by saying that "Professor Elmer Gates, a psychologist who has for several years been making elaborate studies both in Washington and Philadelphia, has added not a little to our knowledge of the developments of the brain and the relation of particular parts of the brain to thought and emotion and the use of particular parts of the body." The view indeed is not new, but the confirmation given by Prof. Gates researches is very interesting" He then quotes language from Dr. Julius Althaus as to the supposed seat of mental activity in the brain, which embodies a general statement of the little knowledge we have on the subject. The question naturally arises as to the alleged researches of Dr. Gates, and the extent to which they have confirmed our hypotheses on this subject, and if so, as to where they were published? The editor does not tell us. This is a pity, for assertions without authority are useless to science. Is there any connection between these researches and the alleged vivisection of idiots recounted in the article we first quoted? The name signed to the latter is not that of Dr. Gates, so we are quite in the dark. A journal which publishes an article by Sir Wm. Dawson, and writes up the Universities, ought to give us more light no these wonderful researches.

—It is again proposed that the American Association for the Advancement of Science meet in San Francisco in the near future. The Board of Supervisors of that city are said to have extended an invitation to visit the city in 1897. The Association has had many such invitations, and they would have been accepted had the railroad authorities been willing to place their rates within reach of the members. The authorities of San Francisco have, however, this time included in their invitation the British and Australian Associations, and we are informed that the British members will have free or nominal transportation via the Canadian Pacific R. R. It is said that the Dominion of Canada will make an appropriation towards defraying their transportation expenses. Perhaps our Congress would be willing to make an appropriation for securing the transportation of our own members. The amount will not

exceed the outlay on funeral solemnities annually expended by it. Such meetings tend to bring about amicable relations among the living, and to promote the interest in and distribution of knowledge. It might be good politics if the Canadian Boundary and Venezuelan questions should be still on hand in 1897.

RECENT LITERATURE.

Petrology for Students: An Introduction to the Study of Rocks under the Microscope, by Alfred Harker, University Press, Cambridge. MacMillan & Co., New York, 1895. Pp. vi and 306; figs. 75; price $2.00.

This volume of the Cambridge Natural Science Manuals will be heartily welcomed by teachers and students of geology in all English-speaking countries. It presupposes a knowledge of the microscopical features of minerals, and consequently deals only with rocks. These the author divides into Plutonic, Intrusive, Volcanic and Sedimentary rocks. Under each head the general characteristics distinguishing each of the several rock classes are briefly mentioned, and descriptions of the different rock types embraced in each group are given. First come descriptions of the constituents of each rock, then follows a statement of its pecularities of structure. The principal varieties are next mentioned, and abnormal, structural and chemical forms are briefly described. The book concludes with chapters on thermal and dynamic metamorphism and one on the crystalline schists.

Of course, the treatment of the different subjects discussed is necessarily very brief, nevertheless it is full enough in most cases to give the student beginning petrography a very good view of the field. A specially important feature of the work is the large list of references to articles written in English. With this book at hand, students will no longer be required to wait until they have mastered German before beginning the study as heretofore been the case. While by no means exhaustive, the present volume will serve as an excellent introduction to the larger French and German treatises, and will, at the same time, be a good reference book for geologists who do not desire to make a specialty of microscopic lithology.—W. S. B.

Crystallography, a Treatise on the Morphology of Crystals, by N. Story-Maskelyne, Oxford, Clarendon Press, 1895. New

York: MacMillan & Co. Pp. xii and 512; figs. 597, pl. viii; price, $3.50.

This "Crystallography" is a real addition to the literature of the subject that it treats. Its appearance reminds one strongly of Groth's "Physiographische Krystallographie," although the book is by no means a reproduction of the German treatise. The latter discusses the subject from the side of solid symmetry, whereas the former deals with it rather from the analytical point of view. The first 187 pages of the volume treat of the general relations of crystal planes and of zones. The next 200 pages take up the six crystal systems beginning with the cubic, and discuss in order the holosymmetrical and the merosymmetrical forms, combination of forms and twinned forms. Chapter VIII, embracing pages 388–463, is devoted to crystal measurements and calculations, and the final chapter to the projection and drawing of crystals. The plates show the projection of the poles of the most general form and of its derived hemihedral and tetartohedral forms in each system.

It is almost needless to state that the work of the author is based exclusively on the system of indices, known generally as the Miller system. Not only are the faces of crystal forms studied through the aid of the spherical projection, but the individual planes are discussed solely in terms of their normals. No reference is made to other systems of notation, nor to other methods of projection than those elaborated. The book might have been of a little more practical value had the author at least referred to other systems, but its unity might have suffered. As it is, the volume is a very complete exposition of crystallography from the Miller standpoint, and it will, without doubt, prove of inestimable value in popularizing this—the most beautiful method of studying the subject. Of course, the treatment is purely mathematical, but the mathematics used are simple enough to be understood by any one acquainted with the methods of spherical geometry. To the student of minerals too much emphasis will seem to be placed on the theoretical aspect of the development of crystal forms, but to the specialist in crystallography, the emphasis will appear to be placed just where it belongs—on the possibility of deriving all possible symmetrical polyhedrons from certain simple abstract notions concerning pairs of planes, at the basis of which is the principle of the rationality of the indices.

There is no doubt that the treatise before us will appeal less strongly to the student of forms than it will to one of analytical proclivities. Nevertheless it is needed even by the former, if, for no other reason,

because it will impress him more strongly than ever with the exactness with which nature constructs her inorganic structures. With Dr. Williams' little book to develop the imagination of the beginner in crystallography and to interest him in the science, and the present volume to carry him on to a very thorough understanding of the relationships of crystal forms, the English-reading student-world is as well, if not as bountifully, supplied with text books on the subject as are the students of any European country.

The authors discussions are all logically developed, and all his statements are clear and simple. The figures are well drawn and the subjects they illustrate are well selected.—W. S. B.

Elementary Physical Geography, by Ralph S. Tarr. New York: MacMillan & Co., 1895. Pp. xxxi and 488; figs. 267, plates and maps 29; price $1.40.

The most striking features of Prof. Tarr's book are the freshness and wealth of its illustrations and the excellence of its typography. The volume is just what its title indicates, except that perhaps the treatment of its subject matter is a little more inclined toward the side of physiography than toward physical geography. The book is indeed elementary—more so than one would wish, sometimes; at other times it is elementary in the statement of the facts described, while leaving their causes unexplained, where a word or two might have avoided a difficulty which the teacher will surely meet with in discussions with his brightest scholars. In the arrangement of material, some fault can easily be found, but, as the author himself declares, the treatment is, "in many respects, experimental." In spite of these criticisms, the experiment is a success.

The volume is divided into three parts, with four appendices and a very good index. The first part deals with the air. It includes chapters on the earth as a planet, the atmosphere in general, distribution of temperature in the atmosphere, its general circulation, storms, its moisture, weather and climate, and the geographic distribution of plants and animals. Why the first and last chapters included in this part are discussed here is not quite plain. Part second deals with the ocean. It embraces chapters on the ocean in general, waves and currents and tides. Part third treats of the land and its features. A general description of the earth's crust is discussed in the opening chapters. Then follow chapters on denudation, the topographic features of the surface, river valleys, deltas, waterfalls, lakes, etc., glaciers, the coast line, plateaus and mountains, volcanoes, earthquakes, etc.,

man and nature and economic products. The appendices include one on meteorological instruments, methods, etc., one on maps and one containing suggestions to teachers. The last is a list of questions on the text. At the end of each chapter is a list of reference books, with their titles and prices. This is not of much value to the student, but is convenient for the teacher. A list of articles to be found in *Nature*, *Science*, the *Popular Science Monthly*, and similar periodicals might have been of more value in an elementary treatise. However, the plan of referring students to original articles on the subjects discussed is commendable. We can not dismiss the book without another reference to the many really excellent illustrations and charts it contains. The former are, without exception, fresh and new, well chosen to illustrate the author's points and well executed from the bookmaker's standpoint. Many of the charts are original. The volume is, on the whole, the most attractive that we have seen on the subject it treats, and its attractiveness is not at the expense of scientific accuracy. We can safely predict a general adoption of the book as a text in many high schools and academies, and we shall be mistaken if it is not used in some of our colleges, where the instructor desires an *aid* in his work rather than a *substitute* for work.—W. S. B.

Gray's Synoptical Flora of North America.—In 1835 or 1836, Dr. John Torrey planned a Flora of North America, with which Dr. Gray soon became identified, and, in July, 1838, the first part (Ranunculaceæ to Caryophyllaceæ) was published; a little later (October, of the same year), the second part appeared, and in June, 1840, the third and fourth parts were issued, completing Vol. I, the Polypetalæ. As will be remembered, Volume II was not completed, a portion appearing in 1841, and the work being suspended at the end of the Compositæ in 1843 (February). Here the work stopped for many years, and was resumed in 1878 by Dr. Gray (Dr. Torrey having died five years earlier) under the slightly different title of A *Synoptical* Flora of North America. In this volume the Gamopetalæ were completed; in 1884, the Compositæ and preceding families, since whose elaboration more than forty years had passed, were revived. Then shortly afterwards, 1888, came the death of Dr. Gray, followed, in 1892, by the death of Dr. Watson, before the publication of other parts.

In October, 1895, Dr. B. L. Robinson issued the first fascicle of the revision of Vol. I of the Flora, a little more than fifty-seven years since the appearance of the corresponding fascicle. This includes the polypetalous families—Ranunculaceæ to Frankeniaceæ. It includes much

of Dr. Gray's work, to which is added something of Dr. Watson's work, to which we have now added the results of Dr. Robinson's studies.

With such a history, stretching back as it does through more than half a century, it is not to be wondered at that the work is conservative to a marked degree. The sequence of families can differ little from that adopted nearly sixty years ago, and in this fascicle the citation of authorities, the matter of nomenclature, etc., have been made to conform as far as possible to the treatment accorded them seventeen years ago. This extreme conservatism is to be regretted, since science is more productive just as its followers are least tied by the traditions of the past. Yet, with all its conservatism, the Synoptical Flora will be invaluable, and every systematic botanist will hope that health and strength may not fail the present editor before his task is completed.

—CHARLES E. BESSEY.

The Natural History of Plants.[1]—About seven years ago the eminent professor of botany in the University of Vienna, gave to the botanical world a book under the title *Pflanzenleben*, with which botanists soon became familiar as a most useful work. Some time ago the welcome announcement was made that the work was to be translated and brought out simultaneously in England and America. This has now been accomplished, and the result is before us in four good sized volumes, each called a "half-volume," which are attractive externally and internally. On comparing the translation, as brought out by Messrs Holt & Co., with the original, it must be conceded that the former is the by far better done, both in the clearness of text and the perfection with which the printer has brought out the illustrations. The colored plates are especially well done, being printed from the originals by the Bibliographische Institut of Leipzig.

For those who have not seen the original, it may be well to say that it presents in a readable manner (in a *popular* manner, we might say, if the word had not been so dreadfully abused) the main facts as to the structure, biology, and physiology of plants. It is not a text book for daily conning by the student, but it is rather a most interesting work to

[1] *The Natural History of Plants*, their forms, growth, reproduction and distribution, from the German of Anton Kerner von Marilaun, Professor of Botany in the University of Vienna, by F. W. Oliver, M. A., D. Sc. Quain Professor of Botany in University College, London, with the assistance of Marian Busk, B. Sc., and Mary F. Ewart, B. Sc. With about 1000 original woodcut illustrations and 16 plates in colors. New York: Henry Holt & Company, 2 vols., large 8vo. pp. 777 and 983.

be read by not only the botanist, but by every intelligent man and woman who would know something of the deeper problems with which modern botany concerns itself. The topics noted in the table of contents will give some idea of the scope of the work as follows: The study of plants in ancient and modern times; The living principle in plants; Absorption of nutriment; Conduction of food; Formation of organic matter from the absorbed inorganic food; Metabolism and transport of materials; Growth and construction of plants; Plant forms as completed structures; The genesis of plant offspring; The history of species.

A single quotation taken from the opening chapter may serve to show the delightful style in which the work is written: "Some years ago, I rambled over the mountain district of north Italy in the lovely month of May. In a small sequestered valley, the slopes of which were densely clad with mighty oaks and tall shrubs, I found the flora developed in all its beauty. There, in full bloom, was the laburnum and manna-bush, besides broom and sweet-brier, and countless smaller shrubs and grasses. From every bush came the song of the nightingale, and the whole glorious perfection of a southern spring morning filled me with delight. Speaking, as we rested, to my guide, an Italian peasant, I expressed the pleasure I experienced in this wealth of laburnum blossoms and chorus of nightingales. Imagine the rude shock to my feelings on his replying briefly that the reason why the laburnum was so luxuriant was that its foliage were poisonous, and goats did not eat it; and that though no doubt there were plenty of nightingales, there were scarcely any hares left. For him, and, I dare say, for thousands of others, this valley clothed with flowers was nothing more than a pasture ground, and nightingales were merely things to be shot.

"This little occurrence, however, seems to me characteristic of the way in which the great majority of people look upon the world of plants and animals. To their minds, animals are game, trees are timber and firewood, herbs are vegetables (in the limited sense), or, perhaps, medicine or provender for domestic animals, whilst flowers are pretty for decoration. Turn in what direction I would, in every county I travelled for botanical purposes, the questions asked by the inhabitants were always the same. Everywhere I had to explain whether the plants I sought and gathered were poisonous or not; whether they were efficacious as a cure for this or that illness, and by what signs the medicinal or otherwise useful plants were to be recognized and distinguished from the rest."—CHARLES E. BESSEY.

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General Notes.

PETROGRAPHY.[1]

The Origin of Adinoles.—Hutchings[2] has discovered a contact rock at the Whin Sill, England, which, in the author's opinion, represents an intermediate stage in the production of an adinole from a fragmental rock. It contains corroded clastic grains of quartz and feldspar in an isotropic base containing newly crystallized grains of quartz and feldspar. The isotropic material is derived from the clastic grains by the processes of contact metamorphism, whatever they may be, as grains of quartz are often seen with portions of their masses replaced by the substance. The rock has begun its recrystallization from the isotropic material produced by solution or fusion of the original grains, but the process was arrested before the crystallization was completed. The paper concludes with some general remarks on metamorphism. The author thinks that the statement that in granite contacts no transfer of material takes place has not yet been proven true. He also thinks that more care should be taken in ascribing to dynamic metamorphism certain effects that may easily be due to the contact action of unexposed dioritic or granitic masses.

Notes from the Adirondacks.—The limestones, gneisses and igneous instrusives of the Northwestern Adirondack region are well described by Smyth.[3] The intrusions consist of granites, diorites, gabbros and diabases. The gabbro of Pitcairn varies widely in its structure and composition, from a coarse basic or a coarse, almost pure feldspathic rock to a fine grained one with the typical gabbroitic habit.

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Geological Magazine, March and April, 1895.

[3] Bull. Geol. Soc. Amer., Vol. 6, p. 263.

Compact hornblende is noted as an alteration product of its augite. Where in contact with the limestones the gabbro has changed these rocks into masses of green pyroxene, garnet, scapolite and sphene. A second variety of the gabbro is hypersthenic. A third variety is characterized by its large zonal feldspars composed of cores of plagioclase surrounded by microperthite, although crystals of the latter substance alone abound in some sections. The ferromagnesian components are rare as compared with the feldspars. Nearly all specimens of these rocks are schistose, and all of the schistose varieties exhibit the cataclastic structure in perfection. Analysis of the normal (I) and of the microperthitic or acid (II) gabbros yielded :

	SiO_2	Al_2O_3	FeO	MgO	CaO	K_2O	Na_2O	H_2O	Total
I	57.00	16.01	10.30	1.62	6.20	3.53	4.35	.15	= 99.16
II	65.65	16.84	4.01	.13	2.47	5.04	5.27	.30	= 99.71

Near the contact with the limestone the gabbro is finer grained than elsewhere. Pyroxene is in larger grains than in the normal rock, but the feldspar is in smaller ones. The limestone loses its banding and is bleached to a pure white color. Between the two rocks is a fibrous zone of green pyroxene and wollastonite, together with small quantities of sphene and garnet and sometimes scapolite and feldspar. The red gneisses, common to that portion of the region studied which borders on the gabbro, are thought by the author to be largely modified portions of the intrusive rock.

The Eastern Adirondacks have been studied by Kemp.[4] The limestones of Port Henry consist of pure calcite, scattered through which are small scales of graphite, phlogopite and occasionally quartz grains, apatite and coccolite. This is cut by stringers of silicates that are granitic aggregates of plagioclase, quartz, hornblende and a host of other minerals. Ophicalcite masses are also disseminated through the limestones, and these are also penetrated by the silicate stringers. Merrill[5] has shown that the serpentine of the ophicalcite is derived from a colorless pyroxene. The schists associated with the limestones are briefly characterized by the author. At Keene Center a granulite was found on the contact of the ophicalcite with anorthosite.

Hornblende Granite and Limestones of Orange Co., N. Y.—Portions of Mts. Adam and Eve at Warwick, Orange Co., N. Y., are composed of basic hornblende granite that is in contact with the white

[4] Ibid, p. 241.

[5] Cf. American Naturalist, 1895, p. 1005.

limestone whose relations to the blue limestone of the same region have been so much discussed. The granite contains black hornblende, a little biotite, and so much plagioclase that some phases of it might well be called a quartzdiorite. Allanite and fluorite are also present in the rock, the former often quite abundantly. As the granite approaches the limestone it becomes more basic. Malacolite, scapolite and sphene are developed in it in such quantity, that immediately upon the contact the normal components of the granites are completely replaced. On the limestone side of the contact the rock becomes charged with silicates, the most abundant of which are hornblende, phlogopite, light green pyroxenes, sphene, spinel, chondrodite, vesuvianite, etc. The contact effects are similar in character to those between plutonic rocks and limestones elsewhere. The blue and the white limestones are regarded as the same rock, the latter variety being the metamorphosed phase.[6]

An Augengneiss from the Zillerthal.—The change of a granite porphyry into augengneiss is the subject of a recent article by Fütterer.[7] The rocks are from the Zillerthal in the Alps. The gneisses are crushed and shattered by dynamic forces until most of the evidences of their origin have disappeared. The original phenocrysts have been broken and have suffered trituration on their edges, while new feldspar, quartz, malacolite and other minerals have been formed in abundance. The groundmass of the gneiss is a mosaic whose structure is partially clastic through the fracture of the original components and partially crystalline through the production of new substances. The author's study is critical, and, though he treats the described rocks from no new point of view, he discusses them with great thoroughness, calling attention at the same time to the important diagnostic features of dynamically metamorphosed rocks.

Petrographical News.—Ransome[8] has discovered a new mineral, constituting an important component of a schist occurring in the Tiburon Peninsula, Marin Co., Cal. The other components of the schist are pale epidote, actinolite, glaucophane and red garnets. The new mineral, lawsonite, is orthorhombic with an axial ratio .6652 : 1 : .7385, a hardness of 8 and a density 3.084. The axial angle is $2V = 84° 6'$ for sodium light. Its symbol is $H_4 Ca Al_2 Si_2O_{10}$.

[6] J. F. Kemp and Arthur Hollick: N. Y. Acad. Sci., VII, p. 638.
[7] Neues Jahrb. f. Min., etc., B.B. IX, p. 509.
[8] Bull. Geol. Soc. Amer., Vol. 1, p. 301.

Fuess[9] has perfected an attachment for the microscope which enables an observer to enclose with a diamond scratch any given spot in a thin section, so that it may be easily identified for further study.

Marsters[10] describes two camptonite dykes cutting white crystalline limestones near Danbyborough, Vt. They differ from the typical camptonite in being much more feldspathic than the latter rock. They moreover, contain but one generation of hornblende, corresponding to the second generation in the typical rock, and but few well developed augite phenocrysts, although this mineral is found in two generations.

A portion of Mte. S. Angelo in Lipari consists of a porous yellowish pyroxeneandesite containing grains and partially fused crystals of cordierite, red garnets and dark green spinel.[11]

Cole[12] declares that the " hullite " described by Hardman as an isotropic mineral occurring in the glassy basalts of Co. Antrim, Ireland, is in reality an altered portion of the rock's groundmass, and is no definite mineral substance.

The same author[13] describes the old volcanoes of Tardree in Co. Antrim as having produced rhyolitic lavas instead of trachytic ones as has generally been stated.

GEOLOGY AND PALEONTOLOGY.

On the Species of Hoplophoneus.—Four species of *Hoplophoneus* have already been described ; *H. cerebralis* Cope, *H. oreodontis* Cope, *H. primaevus* Leidy and Owen, *H. occidentalis* Leidy. *Dinotomius atrox* will be shown to be a synonym of the latter species. To these may be added *H. robustus* and *H. insolens* herein described. The following key may be valuable in determining the species from a few characters.

A. Skull small, occiput nearly vertical.

  a. Superior sectorial with large anterior basal cusp.

    1. Pms. $\frac{2}{}$ *H. cerebralis* John Day.

  b. Superior sectorial with incipient anterior basal cusp.

[9] Neues Jahrb. f. Min., etc., 1895, I, p. 280.

[10] Amer. Geol., June, 1295, p. 368.

[11] Bergeat: Neues Jahrb. f. Min., etc., 1895, II, p. 148.

[12] Belfast Nat. Field Club Proceedings, 1894–5.

[13] Geol. Magazine, No. 373, p. 303.

2. Pms. $\frac{3-2}{2}$ pm. 2 reduced or absent *H. oreodontis* White River.
3. Pms. $\frac{3}{2}$ *H. primaevus* White River.

B. Skull large, occiput overhanging.

Superior sectorial with incipient anterior basal cusp.

4. Pms. $\frac{3-2}{2}$ pm. 2 reduced or absent *H. robustus* White River.
5. Pms. $\frac{3}{2}$ *H. insolens* White River.
6. Pms. $\frac{2}{2}$ inferior sectorial with no posterointernal cusp, heel reduced, *H. occidentalis* White River.

Hoplophoneus occidentalis Leidy.

In The Extinct Fauna of Dakota and Nebraska (1869) Leidy described two fragments of a mandible which he thought indicated a species larger than *Hoplophoneus primaevus* and to which he gave the name *H. occidentalis* (*Drepanodon occidentalis*), figuring the specimen in Plate V. No further material was referred to this species until 1894, when Osborn and Wortman in describing a collection of White River fossils in the Bulletin of the American Museum of Natural History determined two specimens as *H. occidentalis*, giving measurements of the more important bones of the skeleton in comparison with those of *H. primaevus*. While pursuing my studies in the American Museum through the kindness of these gentlemen, I found that a complete mandible of specimen No. 1407 from the Oreodon Beds agrees in every particular with Leidy's type, which I have had the privilege of examining in the Philadelphia Academy. A drawing of the mandible accompanied by a faithful copy of Leidy's figure is given in the accompanying plate. Associated with the mandible are several vertebrae and portions of limb bones showing the skeleton to be much larger than the specimen previously determined as *H. occidentalis* in the American Museum Bulletin. They however, agree, as does also the mandible, with *Dinotomius atrox* described by Dr. Williston in the Kansas University Quarterly, January, 1895, from a fine skull and nearly complete skeleton. This specimen which I had the pleasure of seeing last summer I now have no hesitation in referring to *H. occidentalis*. It makes possible the determination of the skeletal characters and affinities, and the restoration promised by Dr. Williston will complete our knowledge of this species. The following measurements are taken from the Kansas University Quarterly.

Length from inion to premaxillary border . . .	260 mm.
Width of zygomata	145 "
Length of mandibular ramus	164 "

Length of humerus	240 "
Width of distal end of humerus	73 "
Length of tibia	237 "
Width of proximal end of tibia	61 "
Width of distal end of tibia	41 "

The relation of *H. occidentalis* to *Eusmilus dakotensis* Hatcher, published in the December NATURALIST, is at once apparent. By comparison with the excellent figure by Mr. Weber, republished by permission in the accompanying plate, it will be seen that *H. occidentalis* stands directly ancestral to *E. dakotensis*, the dentition agreeing very strikingly in the characters emphasized by Mr. Hatcher, but differing in showing an additional incisor and premolar and the presence of a heel on the sectorial. In *Eusmilus bidentatus* Filhol, the type of the genus, the heel is present.

Hoplophoneus insolens sp. nov.

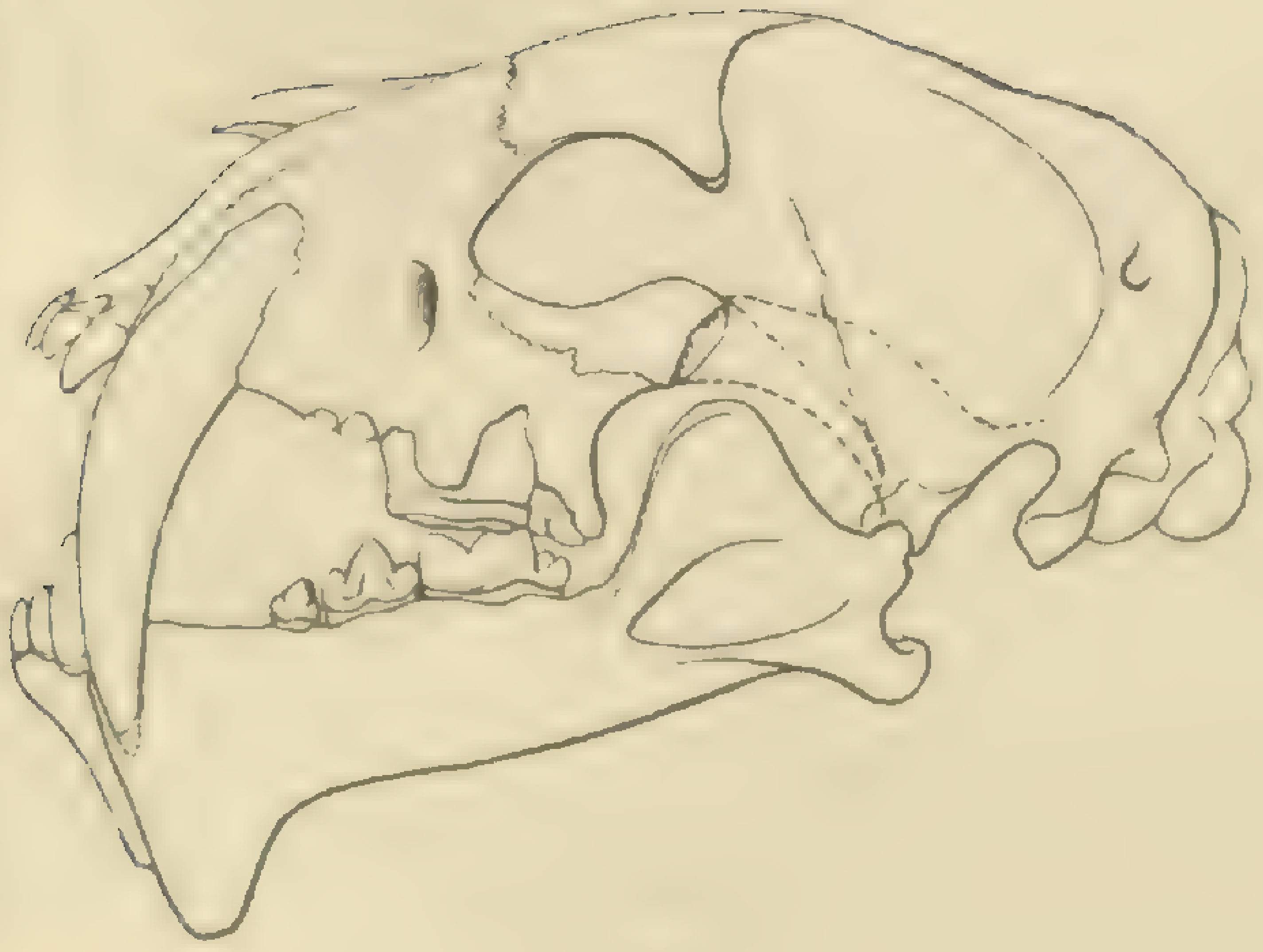
The determination of the characters of *H. occidentalis* makes it obvious that the skeleton determined as such by Osborn and Wortman is a new species. A complete skeleton (number 11,022) and a second specimen with the most of the limb bones and a skull lacking the mandible (number 11,372), both in the Princeton Museum, enable me to determine the skull of this species, a character which is lacking in the American Museum specimen. The particularly close agreement in the size of the skeletons makes either of them typical, consequently I give the measurements already published in the American Museum Bulletin along with measurements from the Princeton specimen as indicative of the size of the species.

The skull of *H. insolens* is long and low, the postorbital constriction very marked, sagittal crest slightly concave, the occiput overhanging and concave from side to side, the posttympanic process is long and massive approaching the postglenoid process and being produced as far inferiorly. The limb bones have stout shafts and relatively small extremities.

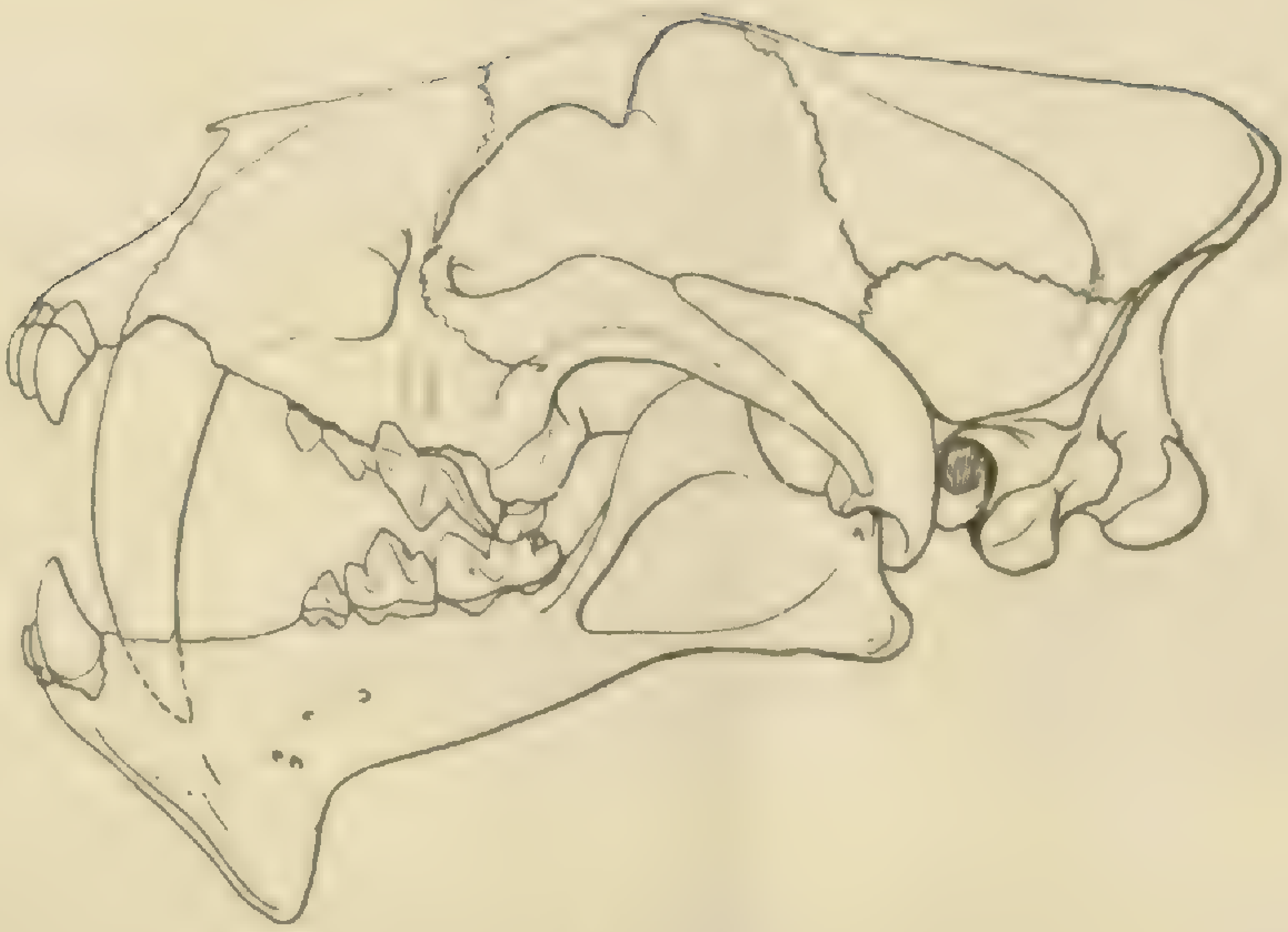
Dentition: I ^{3}, C $\frac{1}{1}$, Pm $\frac{2}{2}$, M $\frac{1}{1}$; the second upper premolar which is variable in the genus usually being absent in this species.

Length of skull, condyles to premaxillary border	190 mm.
Length of humerus	200 "
Length of ulna	212 "
Length of radius	160 "
Length of femur	250 "
Length of tibia	188 "
Length of pelvis	210 "

PLATE I.



3

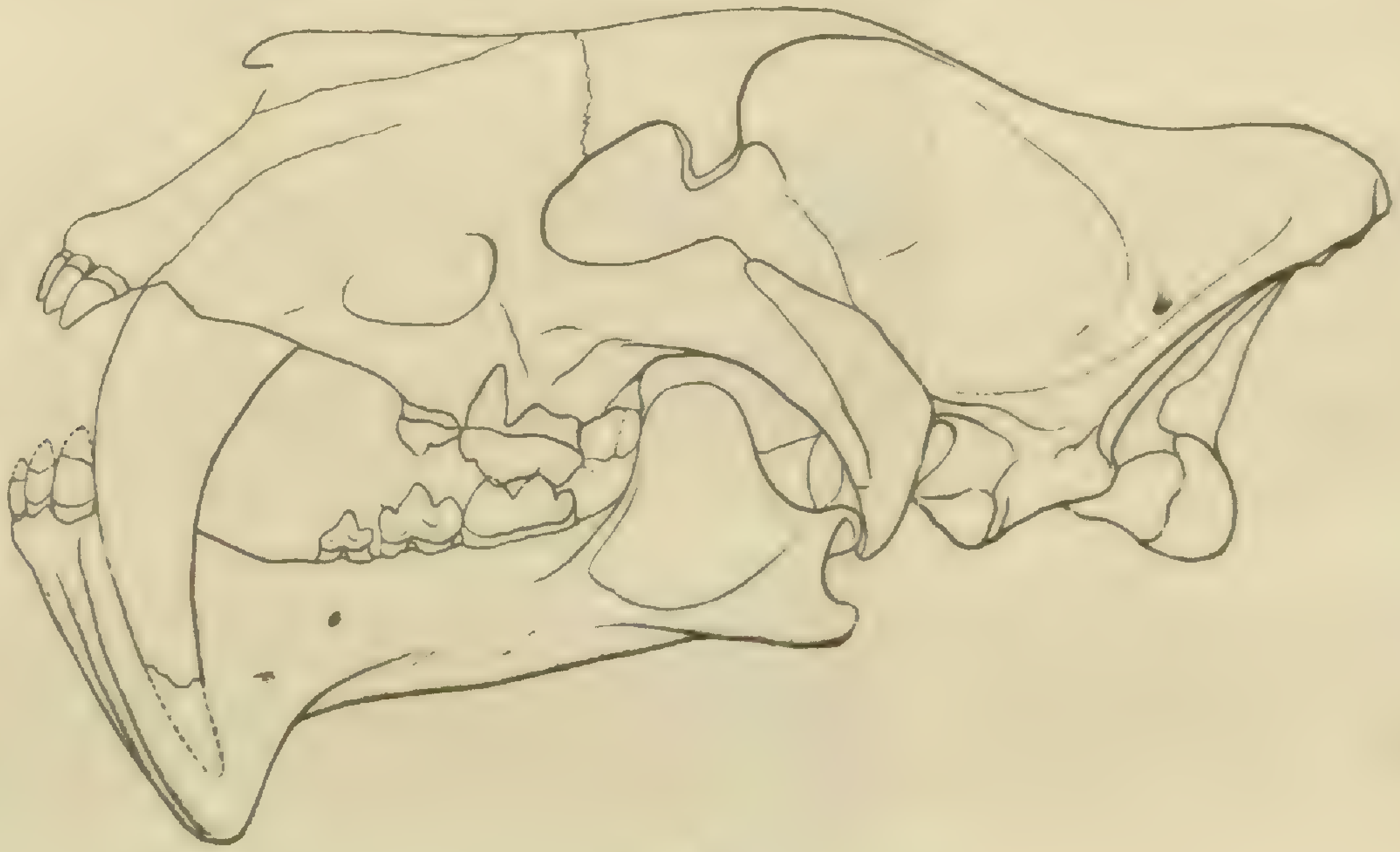


4

2

5

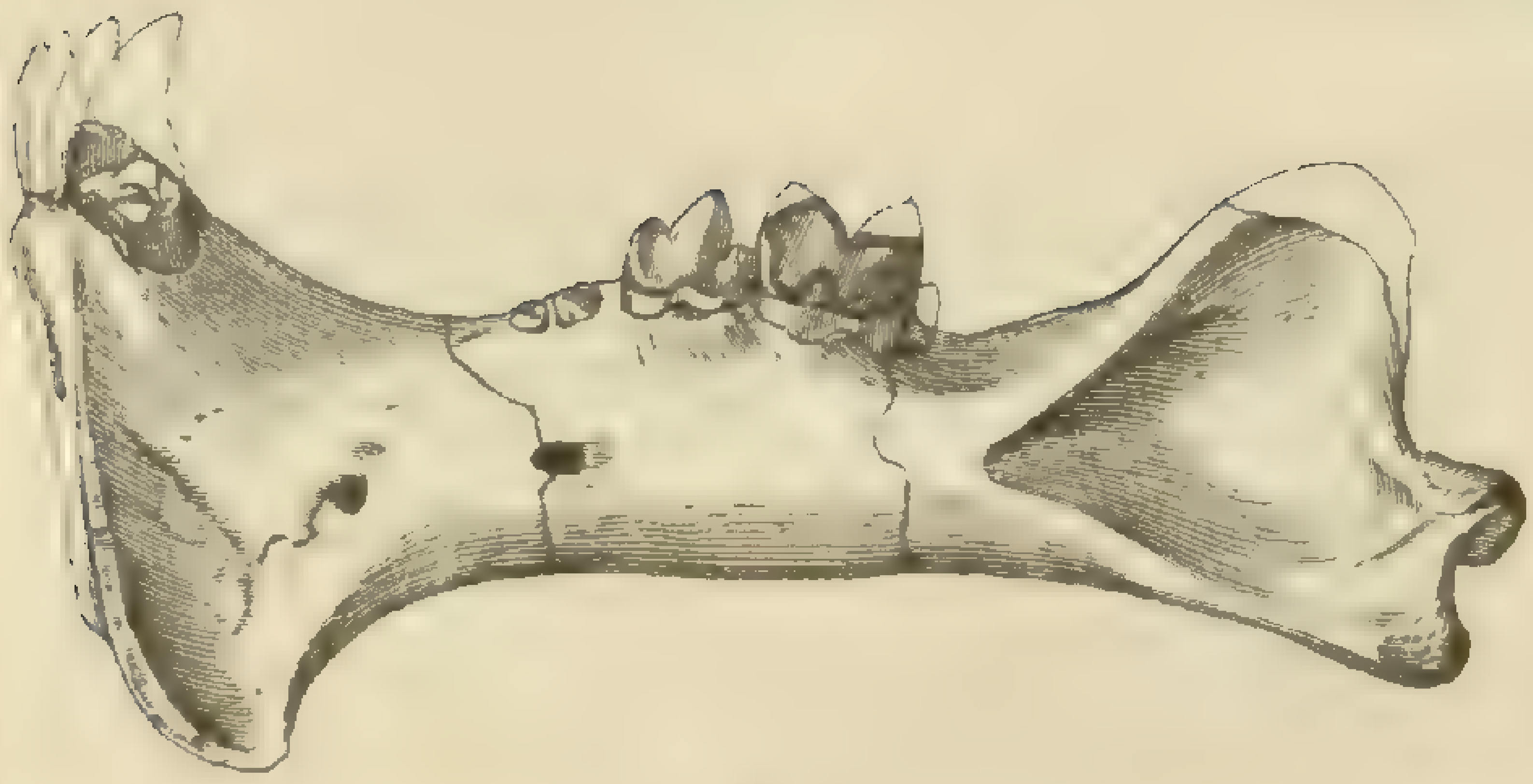
1



6

Adams on the Species of Hoplophoneus.

PLATE II.



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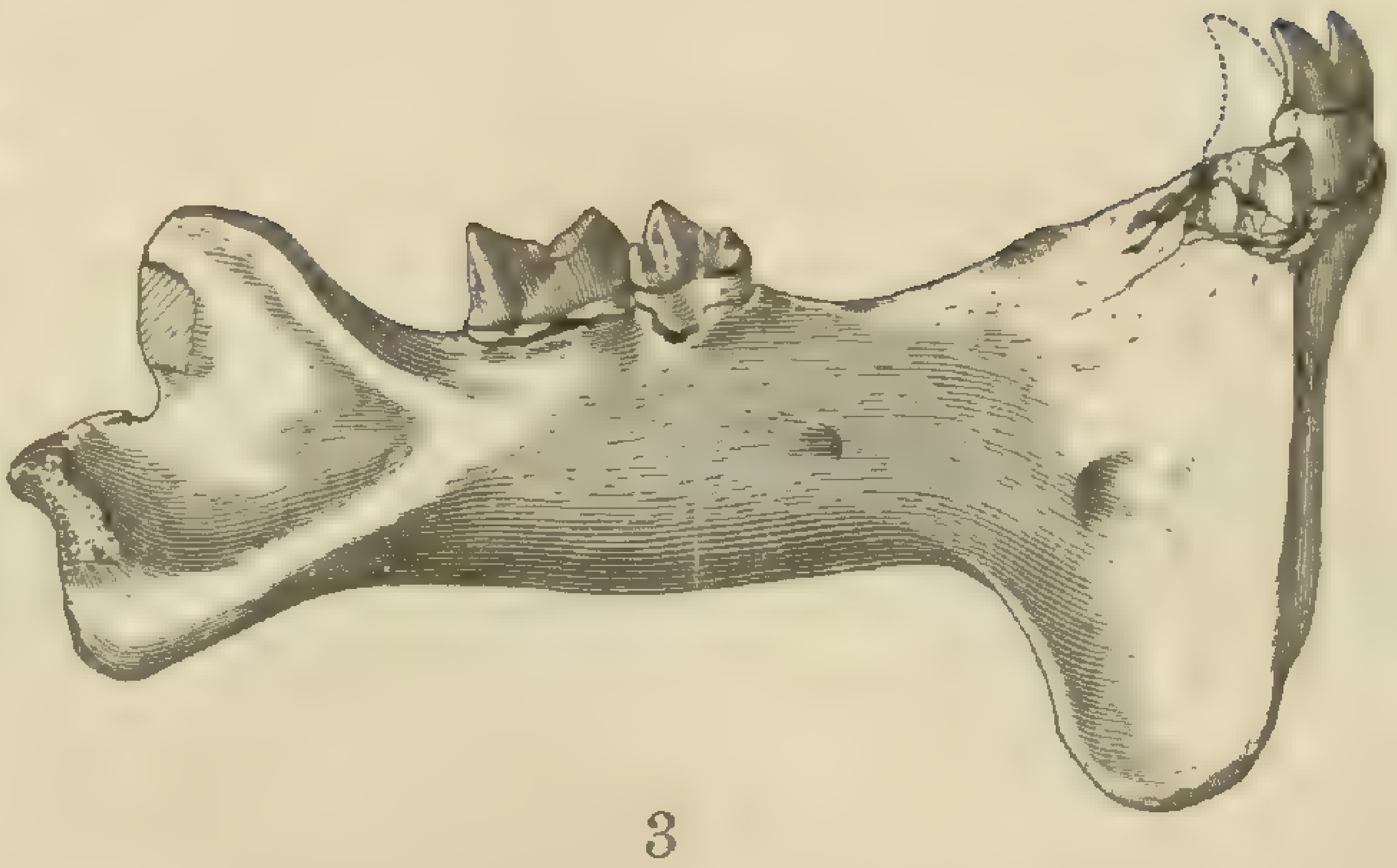
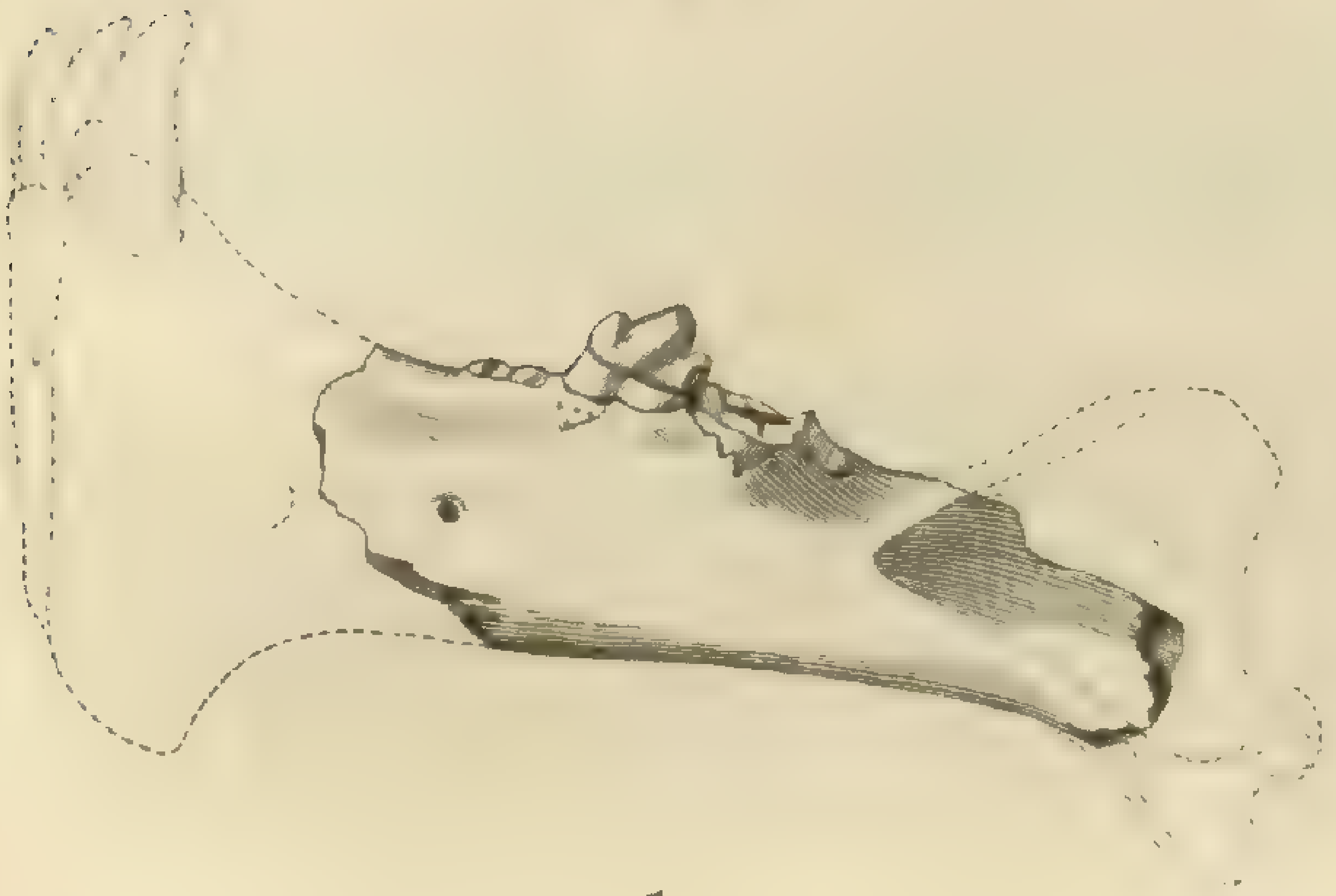


3



1

4

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6

7

Adams on the Species of Hoplophoneus.

Hoplophoneus primaevus Leidy and Owen.

The original type of this species is figured in the The Ancient Fauna of Nebraska (1853). Later Leidy figured two skulls in the Extinct Fauna of Dakota and Nebraska, remarking that the larger one might be the skull of an old male, and that the original type was somewhat intermediate in size. The determination of the variation due to sexual characters seems impossible in the case of the extinct cats. However, the material which is available, shows that there are two types represented by these two skulls, the skeletons referable to the types differing more markedly than the skulls. Inasmuch as Leidy's original type agrees more closely with the smaller one, the difference being about such as is presented in any of the species of Machairodonts, it is taken as representative of *H. primaevus.* In the Princeton collection there is a fairly complete skull (number 11,013) and two nearly complete skeletons (numbers 10,741 and 10,934), with the latter skeleton there is also most of the skull. This makes it possible to correlate the skull with the skeletons and give the measurements of the species. The skull is short and high in the frontal region, the orbit horizontally oval, the posttympanic process short, the glenoid drooping considerably below it.

The skeleton is not rugose and the limb bones have slender shafts as in *Dinictis felina.* The dental formula is I $\frac{3}{3}$, C $\frac{1}{1}$, Pm $\frac{3}{2}$, M $\frac{1}{1}$; the second superior premolar probably being constantly present.

Length of skull, condyles to premaxillaries, Leidy's type (approximately)	150 mm.
Length of humerus	160 "
Length of ulna	163 "
Length of radius	122 "
Length of femur	185 "
Length of tibia	143 "

Hoplophoneus robustus sp. nov.

This species is proposed as representative of Leidy's second type of *H. primaevus.* It has its most perfect type in the skeleton and skull (No. 650) determined as *H. primaevus* by Osborn and Wortman, the measurements of which were published in the American Museum Bulletin, Vol. VI, 1894, p. 228, along with those of *H. insolens* (*H. occidentalis*) and which I give here, adding the measurement of the skull. The species is represented in the Princeton collection by specimen

Mo. Bot. Garden,

number 10,647, consisting of a fairly well preserved skull and mandible together with a humerus and portions of other limb bones. The skull is relatively large compared with the skeleton. The limb bones are rugose and have stout shafts, being very similar to those of *Dinictis fortis*,[1] and are thus very different from those of *H. primaevus*. Dentition: I $\frac{3}{3}$, C $\frac{1}{1}$, Pm $\frac{2\text{-}3}{2}$, M $\frac{1}{1}$.

Length of skull, condyles to premaxillary border	180 mm.
Length of humerus	170 "
Length of ulna	163 "
Length of radius	132 "
Length of femur	195 "
Length of tibia	160 "
Length of pelvis	180 "

Hoplophoneus oreodontis Cope.

This species is Cope's type of the genus. I introduce it here for the purpose of mentioning a complete skull in the Princeton Museum (number 10,515) which supplements the original type and is, therefore, used here for comparison. The approximate lengths of the femur and tibia are based upon the lengths of these bones associated with the type skull, the epiphyses being lost. Dentition: I $\frac{3}{3}$, C $\frac{1}{1}$ Pm $\frac{2\text{-}3}{2}$, M $\frac{1}{1}$.

Length of skull, condyles to premaxillary border, approximately	135 mm.
Approximate length of femur	120 "
Approximate length of tibia	110 "

Hoplophoneus cerebralis Cope.

This species from the John Day is the smallest of the genus and at the sametime the most peculiar. Cope has pointed out its specific characters as follows: Space for the temporal muscle relatively short; brain capacity large; profile of the face very convex; sagittal crest horizontal; occiput vertical; no paroccipital processes; orbit vertically

[1] In my description of *D. fortis*, American Naturalist, June, 1895, I compared the skeleton with that of *H. occidentalis*, following the description of that species as given by Wortman and Osborn, which the foregoing determination of its skeletal characters shows to be incorrect. *D. bombifrons* which I described at that time I now find to be a synonym of *D. fortis*; the skull described being correlated with the skeleton and portion of a skull of *D. fortis* by means of specimen number 1400 of the American Museum.

oval. Dentition: I 3/, C 1/, Pm 2/, M 1/, the third premolar being much reduced.

Length of skull, condyles to premaxillary border (approximately) . . . . 120 mm.

There are thus six species of *Hoplophoneus*, disregarding *H. strigidens* Cope, which being based upon a fragment of a canine exhibiting a peculiar form, is not characterized by any features which refer it to *Hoplophoneus* rather than any other genus. With the exception of *H. cerebralis* they are all from the White River. They present an interesting series both in the size of the skulls and skelétons. The accompanying series of femora give an idea of the relative characters of the skeletons of the larger members of the genus as regards size and strength. Unfortunately nothing is known of the skeleton of *H. cerebralis*, but judging from the size of the skull it would be the smallest of the series, although probably not much smaller than that of *H. oreodontis*. In restoring the femur of *H. occidentalis* I am indebted to Dr. Williston for information as to its length.

The series of skulls figured in outline when taken in connection with the series of femora give an idea of the relative size of the species. The gradation in size is for the most part comparable with the gradation in size of the skeletons. Each species has shown, from careful comparisons and measurements of all the available material, a limited amount of variation, but in no case losing its identity when both the skull and skeleton are taken into consideration.—Geo. I. Adams, Fellow of Princeton College.

Explanation of Plates.

Plate I.

Fig. 1.—Hoplophoneus cerebralis (after Cope).
Fig. 2.—Hoplophoneus oreodontis (number 10,515 Princeton Museum).
Fig. 3.—Hoplophoneus primaevus (after Leidy).
Fig. 4.—Hoplophoneus robustus (number 650 American Museum).
Fig. 5.—Hoplophoneus insolens (number 11,022 Princeton Museum).
Fig. 6.—Hoplophoneus occidentalis (after Williston).

All $\times \frac{3}{8}$

Plate II.

Fig. 1.—Hoplophoneus occidentalis (Leidy's type).
Fig. 2.—Hoplophoneus occidentalis (number 1,047 American Museum).
Fig. 3.—Eusmilus dakotensis (after Hatcher).

Fig. 4.—Hoplophoneus primaevus.
Fig. 5.—Hoplophoneus robustus.
Fig. 6.—Hoplophoneus insolens.
Fig. 7.—Hoplophoneus occidentalis.

All $\times \frac{3}{8}$

The Goldbearing Quartz of California.—The salient characteristics of the gold quartz veins of California are briefly given by Mr. Waldemar Lindgren in a paper recently published, and the results of his observations are thus summarized :

"The auriferous deposits extend through the state of California from north to south, in an irregular and unbroken line.

"The gold quartz veins occur predominantly in the metamorphic series, while the large granitic areas are nearly barren. The contact of the two formations is not distinguished by rich or frequent deposits."

"The gold quartz veins are fissure veins, largely filled by silica along open spaces, and may dip or strike in any direction.

"The gangue is quartz, with a smaller amount of calcite ; the ores are native gold and small amounts of metallic sulphides. Adjoining the veins, the wallrock is usually altered to carbonates and potassium micas by metasomatic processes.

"The veins are independent of the character of the country rock, and have been filled by ascending thermal waters charged with silica, carbonates and carbon dioxide.

"Most of the veins have been formed subsequent to the granitic intrusions which closed the Mesozoic igneous activity in the Sierra Nevada."

Regarding the origin of the gold, the author speaks with reserve. He points out the possibility of its derivation from the surrounding rocks, which theory, however, is not altogether satisfactory. He then states the following facts and the conclusion based upon them :

"First, the gold quartz veins throughout the state of California are closely connected in extent with the above described metamorphic series and that the large granite areas are almost wholly void of veins, though fissures and fractures are not absent from them.

"Second, that in the metamorphic series the gold quartz veins occur in almost any kind of rock, and that if the country rock exerts an influence on the contents of the veins, it is, at best, very slight.

"Third, that the principal contact of the metamorphic series and the granitic rocks is in no particular way distinguished by rich or frequent deposits.

"It is further apparent that gold deposits have been formed at different periods, though, by far, most abundantly in later Mesozoic times. Some of these later veins may have been locally enriched by passing through earlier impregnations in schist or old concentrations in the sandstones and conglomerates of the metamorphic series, the gold contents of which have, however, only been proved in isolated cases.

"These considerations strengthen the belief that the origin of the gold must be sought below the rocks which now make up the surface of the Sierra Nevada, possibly in granitic masses underlying the metamorphic series." (Bull. Geol. Soc. Am., Vol. 6, 1895.)

Precambrian Sponges.—M. L. Cayeux has published a preliminary note on the spicules of sponges found in the Precambrian beds of Bretagne. The author describes the different forms of the spicules, gives their dimensions, the mode of fossilization, and the probable causes for their fragmentary condition. The principal conclusions derived by M. Cayeux from his researches are (1) numerous spicules of sponges of various species are found in the Precambrian phtanite formations of Bretagne, and (2) that all the orders of sponges with silicious skeletons are represented in these formations.

A resume of the facts ascertained concerning this interesting fauna is given by the author as follows:

"It is impossible not to be struck by the ensemble of the sponges of the phtanites of Lamballe. Even excluding all the spicules which, although they certainly are sponges, yet are too fragmentary for exact identification, there remains an assemblage of forms which points to a very complex fauna.

"In the light of our present knowledge this fauna appears to be composed of Monactinellidæ, probably abundant, Tetractinellidæ, relatively rare, numerous Lithistidæ, and a few Hexactinellidæ. All the orders of Silicea are represented. The branching off of the sponges is then plainly as early as the base of the Precambrian of Bretagne.

"The oldest beds in which any remains have been found belongs to the Archean of Canada. M. G. F. Matthew has described *Cyathospongia? eozoica* from the Lower Laurentian of St. John (New Brunswick) and *Halichondrites graphitiferus* from the Upper Laurentian of the same region.

"*Cyathospongia? eozoica* may be a species of Hexactinellidæ, and *Halichondrites graphitiferus* must be referred either to Monactinellidæ or to Hexactinellidæ. The authenticity of these fossil sponges has been put beyond a doubt by M. Hermann Rauff.

"All the great groups of silicious sponges do not figure in this assemblage, but the fauna presents this character worthy of note, that the Lithistidæ and the Hexactinellidæ, that is to say, the sponges which have the most complex skeleton occupy a prominent place.

"I have called attention to these Cambrian sponges to show that there is no fundamental difference between the Precambrian and the Cambrian sponge fauna. In the one as in the other, we find already traced, the lines along which the future silicious sponges are developed." (Annales Soc. Geol. du Nord T., XXIII, 1895.)

Embryology of Diplograptus.—A large collection of specimens of Graptolites found near Dolgeville, N. Y., furnishes Mr. R. Ruedeman the data for a paper on the mode of growth and development of the genus Diplograptus. The species, *D. pristis* Hall, and *D. pristiniformis* Hall, appear as compound colonial stocks instead of single stipes, as hitherto known. From his observations the writer infers that the colonial stock was carried by a large air bladder, to the underside of which was attached the funicle. The latter was enclosed in the central disc, and this was surrounded by a verticil of vesicles, the gonangia, which produced the siculæ. Below the verticil of gonangia and suspended from the funicle was the tuft of stipes.

It is evident from the structure of these graptolites that the genus Diplograptus has the combined properties of different groups, and gives valuable hints in regard to their common ancestry. The investigation of Mr. Ruedeman is one of the most important recent acquisitions of paleontologic embryology. (Am. Journ. Sci., 1895, p. 453.)

The Upper Miocene of Montredon.—M. Ch. Deperet has just published the results obtained through the excavations he has been making in the hill of Montredon near Bize (Aude), The fossils which he has collected are found also in the peat beds where they are much broken and slightly worn, and in the white marls where he has found more complete specimens, such as skulls and parts of limbs with the bones in proper relation.

Notwithstanding an abundance of fossils, the fauna of Montredon, until now, was characterized by a paucity of species, comprising only Dinotherium, Hipparion, a Rhinoceros and an undetermined Ruminant. The discoveries of M. Deperet have increased the known vertebrates to twelve. There are, in addition to the animals just mentioned, a wild boar, agreeing with *Sus major* of Leberon ; three ruminants, *Tragocerus amalthœus*, *Gazella deperdita*, and Micromeryx ; three carnivores, *Si-*

mocyon diaphorus, *Dinocyon*, *Hyænarctus arctoides*. This last constitutes, says the author, a true intermediate type between Hyænarctus of the Miocene and the bears of the Pliocene, as *Ursus arvernensis* and *Ursus etruscus*. M. Deperet adds that the discovery of this animal fills a gap by revealing in a precise manner the ancestral relation of the bear type. (Revue Scientif., 1895, p. 375.)

BOTANY.[1]

The Vienna Propositions.—(*Continued from page* 1100, *Vol. XXIX.*)—In a succeeding number of the same journal, Dr. Kuntze replies to the foregong article at some length. A considerable portion of the reply is taken up with personalities. This is not without provocation, for Ascherson and Engler have grievously misrepresented him in more than one place in the foregoing article, e. g., in the matter of his proposed 100-year limitation, and his comparison of the changes required by 1737 and 1753—as one can readily see by glancing at Revisio Generum 3[1]. Indeed, they substantially concede the injustice of their accusation as to Knntze's statement with reference to the changes required by 1753, a few paragraphs beyond, when they discuss their proposed limitation of fifty years. The anonymous correspondent of the *Journal of Botany* who was so pained at the supposed bitterness prevailing in America, is respectfully referred to the pages of the *Oesterreiche Botanische Zeitschrift* for an example of the state of feeling in other lands.

The following extracts will give an idea of Dr. Kuntze's reply.

Of the six propositions of Ascherson and Engler he says: "Numbers 1–4 are not new; No. 5 is a *principium inhonestans*, and No. 6 a supplement to No. 5. The new principle is a year limitation proposal with retroactive force. I had previously proposed a limitation of 100 years only for names sought to be revived in the future, which would only affect old names which are mostly doubtful and undetermined, so that by my proposed limitation, the doubtful cases would be disposed of and greater stability of nomenclature brought about. By the proposition of Messrs. Ascherson and Engler on the other hand, acquired rights would be violated. The gentlemen, indeed, in their last account no longer recognize this right, even as little as the right o political legitimism. These gentlemen now reject also the law o

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

priority, and their proposals have never conformed to the Paris code. One must ask involuntarily what laws Messrs. Ascherson and Engler do recognize in nomenclature at all. With the best intentions, I cannot perceive any trace of a 'Rechtsboden.'"

"The Paris code" he continues, "is in my opinion better than the proposals and deviating principles which Engler, Ascherson and Pfitzer suggest and which they themselves follow only in part. Supposing one followed out the deviating principles honestly and consistently, many more name alterations and complications would result than through following the Paris code."

Since Ascherson and Engler have been at some pains to expose what they deem fundamental errors, one may well suggest a fundamental error upon which they proceed. Their whole argument is based upon the notion that there is a current nomenclature. It is this very notion, indeed, which creates a large part of the opposition to all systematic attempts to bring order into nomenclature. When a systematist goes goes about the work of adjusting the nomenclature of his particular group, current nomenclature does not trouble him at all. There he sets about him with vigor, and even, perhaps, in accordance with rule and principle. But as he looks about him beyond the range of his own group, he feels that it would be very convenient if names could stand as they are in the nearest book at hand, and he becomes conscious of something which he calls current nomenclature. It may be safely affirmed that if Dr. Kuntze had taken up a small group and worked out its nomenclature with the care and thoroughness he bestowed upon all the Phanerogams, no one would have made more than a passing objection, and before long his names would have found themselves current. Who ever said anything about the radical changes made in the nomenclature of the *Uredineæ* when Winter and afterwards Schroeter replaced name after name by the old specific names of *Æcidium* and *Uredo* forms? Very little that Dr. Kuntze has done is more radical than that—and their changes are as current as anything can be said to be at the present day. Before we set about preserving a current nomenclature, we must produce one, and that can only be done by adhering consistently to rules.

As to the propositions made by Escherson and Engler, not much need be said. The 5th and 6th are avowedly only another form of the discredited 4th Berlin thesis. The whole object of the authors seems to be to save their list of eighty-one names—if not by one means then by another. They are as radical as the best of us as far as specific nomenclature is concerned, and one might well suggest that their attitude

towards the eighty-one names they are bent on saving at all hazards, savors quite as much of "legitimism" as anything in the nomenclature controversy. Moreover the propositions are by no means as easy of application as they might appear. The work of restoring prior names has been going on pretty steadily for many years. Since 1891 it has gone on quite rapidly. Are the names restored since the reform movement began to stand, or are we to add a 7th proposition, something like this: "No name recognized since 1891 is to be deemed withdrawn from the operation of the 5th rule?" Then again it must be decided what shall be considered "use" of a name. If a name appears in a work of wide circulation there is a presumption that it has been used more or less. How many other works must cite it to give it validity? And must they cite it with approval, or will citation as a synonym or without comment suffice? What sort of works shall be referred to to ascertain whether a name has been used? Are names used in catalogues and printed lists used? If a writer publish two books, say five years apart, and cite his own names, if one of the books comes within the limit, have the names he quoted from himself been used? Or must some other author use them? The room for individual eccentricity in the application of such a rule is too great to make the rule practicable.

Besides what need is there of pretending to begin the nomenclature of genera with 1753, when in fact it is begun with 1845? As Ascherson and Engler point out, their limitation substantially makes it immaterial whether the nominal starting point is 1753 or 1690. The labored distinction between generic and specific nomenclature amounts to very little. It is only partially true that the alteration of a generic name entails the alteration of the name of every species in that genus. Under the Kew Rule it might, perhaps, but otherwise it can scarcely be said that a change of a generic name burdens the memory any more than the change of a specific name. So long as the distinguishing portion of the binominal remains unchanged, each new binominal does not have to be learned over.

In conclusion, without going into the merits of the controversy between Kuntze and Ascherson and Engler, I may say that Dr. Kuntze never hides behind vague general statements, but supports his assertions by citations and actual instances, so that they may be verified Whether one accedes to Kuntze's conclusions or not, he may always know upon what they are based. It would be much easier to determine the value of the assertions made by his opponents if they were in the habit of doing the same. It is easy to declaim against "disagreeable alterations" and to make insinuations as to the motives of the reform-

ers. But the fact remains that Dr. Kuntze has only attempted to do, a little radically perhaps, for all the flowering plants at one stroke, what monographers had been doing piecemeal in every group of the vegetable kingdom. No one objected to their motives, and few to their alterations. Their alterations became a part of "current nomenclature." Had the reform been conducted haphazard and piecemeal, it would have seemed quite proper to many who now vigorously denounce it.—Roscoe Pound.

The Flora of Ohio.—In the "Catalogue of Ohio Plants" in Vol. VII of the Geology of Ohio, Professor W. A. Kellerman and W. C. Werner make an admirable contribution to our knowledge of the plants of one of the older regions west of the Allegheny Mountains. The catalogue is prefaced by twenty pages or so of historical matter in which we learn that the earliest catalogue of Ohio plants (Miami County) was prepared in 1815 by Dr. Daniel Drake; this was followed in 1818 by a paper on the Scenery, Geology, Mineralogy, Botany, etc., of Belmont County, Ohio, by Caleb Atwater in the *American Journal of Science* (Vol. I), and later, 1831, by Short and Eaton's paper (Southern Ohio) and two by Riddell,—Franklin County, in 1834, and the Flora of the Western States, in 1835, to which a supplement was added in 1836. Then follow lists by Sullivant (1840), Bigelow (1841), Lea (1849), Clark (1852 and 1865), Lapham (1854), Klippart (1858 and 1860), Newberry (1859), Hussey (1872), Beardslee (1874), Wright (1889), besides many short papers in periodicals.

Following the introductory pages one comes at once to the enumeration of plants, in which the arrangement of the families is that of Engler and Prantl, but oddly enough—in *reversed order*. Why the authors gave themselves the trouble to invert the natural sequence is not stated. It is awkward, to say the least. We notice with pleasure that the revised nomenclature has been used, and that all specific names have been decapitalized. Double citations of authorities are given when necessary, and varieties are given as trinomials. Altogether the catalogue is a modern one in plan and execution.

After the Angiosperms, there follow the Gymnosperms, Vascular Cryptogams, Bryophyta, Hepaticæ, Lichenes, Fungi, Algæ and Myxomycetes. Of the last six groups the authors state that the list "must be considered very fragmentary and a mere beginning," yet this is an excellent beginning, of which the State of Ohio needs by no means to be ashamed.—Charles E. Bessey.

The Flora of the Sand Hills of Nebraska.—Mr. P. A. Rydberg has recently published in the Contributions from the U. S. National Herbarium (Vol. III, No. 3) the results of his careful exploration of the Sand Hills of Central Nebraska in the year 1893. Two or three counties in about the center of the sand hill region were selected as the ground to be thoroughly studied, and three months were given to this limited area. Two streams transverse this area, the Middle Loup River and the Dismal River. The former is a rapid stream running down a slope of 8½ to 13 feet to the mile, with hills from 200 to 300 feet high on each side of the rather wide valley (½ to 1½ miles). In its narrower portions the valley is filled with lagoons and swamps, the remains of old river beds. The Dismal River runs through a narrower valley, and the bluffs are higher, ranging from 300 to 600 feet. Away from the rivers Mr. Rydberg found three kinds of sand hills, the first of these are called by him the "barren sand hills," not because they are without vegetation, for they are not, but because they are at present of very little use to man. Here one finds the true Sand Hill vegetation, and when seen from the higher points "the hills appear likes the billows of the ocean."

The Dry Valley Sand Hills constitute the second kind. The hills are long ridges running mostly east and west with long valleys between. The underground drainage is so perfect that little or no water gathers in the valleys, but their rich soil readily yields good crops, or excellent pasturage.

The Wet Valley Sand Hills differ from the last in the greater abruptness of the ridges, which are, in fact, sometimes impassable, and in the less perfect drainage, ponds of water generally occurring at the easterly end of the valleys. In no case is there "surface drainage," every pond being destitute of an outlet. About these ponds grasses grow luxuriantly.

It is evident that the Sand Hill flora is not a homogeneous one. The plants growing along the rivers and about the ponds are very different in character from those which occur on the wooded summits of the "barren sand hills," or the steep slopes of the hills which border the dry and wet valleys. In summing up a discussion of the matter, Mr. Rydberg says: "The most characteristic plants of the sand hills are the four blowout grasses, *Calamovilfa longifolia, Eragrostis tenuis, Redfieldia flexuosa, Muhlenbergia pungens,* of which the first two are found on nearly every sand hill. Next to these the following are the most common or characteristic herbaceous plants:

Andropogon scoparius
Andropogon hallii
Stipa spartea
Stipa comata
Psoralea lanceolata
Psoralea digitata
Carduus plattensis
Opuntia rafinesquii
Euphorbia petaloidia
Euphorbia geyeri
Chrysopsis villosa
Cristatella jamesii
Corispermum hyssopifolium
Croton texensis
Acerates viridiflora
Acerates angustifolia
Acerates lanuginosa
Astragalus ceramicus longifolius
Commelina virginica
Tradescantia virginica
Yucca glauca
Amaranthus torreyi
Frœlichia floridana
Cyperus schweinitzii
Laciniaria squamosa
Cycloloma atriplicifolia
Argemone albiflora

"The most abundant woody plant is *Amorpha canescens*, which is common all over the sand hills. Next comes the Western Sand Cherry (*Prunus besseyi*). On the sand hills around Thedford the third in order is *Ceanothus ovatus*. *Kuhniastera villosa*, which should, perhaps, be classed among the undershrubs, is as common as any of the class. All these belong to the true sand hill flora. Nearly all the other woody plants are confined to the Middle Loup and Dismal River Valleys. A few, as for instance, *Salix fluvialilis*, *Symphoricarpus occidentalis*, *Prunus americana*, *Amorpha fruticosa* are also found in some of the wet valleys."

The other woody plants along the streams are *Cornus stolonifera*, *Ribes floridum*, *Rhus radicans*, *Rosa fendleri*, *Rosa arkansana*, *Ribes aureum*, *Rhus trilobata*, *Acer negundo*, *Fraxinus pennsylvanica*, *F. pennsylvanica lanceolata*, *Populus deltoides*, *Celtis occidentalis*, *Juniperus virginiana*, *Parthenocissus quinquefolia*, *Vitis vulpina*, *Celastrus scandens*, *Rubus occidentalis*, *Ribes gracile*, *Crataegus coccinea*, *Ulmus americana* and *Rhus glabra*.—Charles E. Bessey.

Recent Botanical Papers.—Dairy Bacteriology by Professor H. W. Conn comes to us from the U. S. Department of Agriculture, giving the results of the author's work the past three years.—From the same source we have papers on Grass Gardens and Alfalfa, by Jared G. Smith; Fertilization of the Soil as affecting the Orange in Health and Disease, by H. J. Webber; The Grain Smuts, their Cause and Prevention, by Walter T. Swingle; Water as a Factor in the Growth of Plants, by B. T. Galloway and A. F. Woods; Forestry for Farmers, by B. E. Fernow.—From the Proceedings of the Iowa Academy of

Sciences we have Pollination of Cucurbits, Diseases of Plants at Ames in 1894, and Distribution of Some Weeds in the United States, by Professor L. H. Pammel.—Dissemination of Plants chiefly by their Seeds, is the title of a pamphlet of fifteen pages based upon the specimens collected by the lamented young botanist Miss Mary E. Gilbreth, and after her death presented to Radcliffe College. It will prove to be very suggestive to those who wish to prepare similar collections.—"A Guide to find the names of all wild-growing Trees and Shrubs of New England by their Leaves," and "Ferns and Evergreens of New England," are two pamphlets by Edward Knobel, which deserve to be widely used in the public schools. They consist of good figures of the leaves, which should make it possible for even the non-botanical teacher to direct the attention of children to the trees and ferns. They are sold by Bradlee Whidden of Boston for fifty cents each.—We may notice here the beautiful photogravures of fungi issued by C. G. Lloyd, of Cincinnati, Ohio; the last numbers are *Coprinus comatus*, *Crucibulum vulgare*, *Lycoperdon separans* and *Urnula craterium*.—Professor T. A. Williams has published (Bulletin 43, Agricultural Experiment Station) a paper upon the Native Trees and Shrubs of South Dakota, in which he lists 37 trees and 80 shrubs. Of these, twelve trees and thirteen shrubs are found in all regions of the State. In the Black Hills, a small region including not more than one-eighth of the whole area of the State, no less than eighty-two of the one hundred and seventeen trees and shrubs are found.—Professor MacDougal writes on Botanic Gardens in the October *Minnesota Magazine*. A half tone illustration of the Botanic Institute at Leipzig, and another of the Botanic Garden at Buitenzorg, Java, accompany the paper.

VEGETABLE PHYSIOLOGY.

Changes Due to an Alpine Climate.—For ten years M. Gaston Bonnier, of Paris, has carried on experiments in various parts of France to determine just what changes occur in plants when they are transported from the lowlands to high elevations. These are described in a bulky paper in *Annales des Sciences Naturelles*: *Botanique*, Sé. VII, T. 20, Nos. 4, 5, 6, entitled Recherches expérimentales sur l'adaptation des plantes au climat alpin. Plants of many genera were removed from the plains, the roots or root-stocks divided into equal parts, and these parts set in similar soil and situations at various elevations, up to several thousand metres, in the Alps and the Pyrenees,

and examined from time to time for anatomical and physiological changes. These soon made their appearance and were as follows, the changes in the plants exposed to the alpine conditions being attributed principally to (1) More intense light; (2) Drier air; (3) A lower temperature. *Change of form and structure:* (1) The subterranean parts as a whole are relatively better developed than the parts above ground. (2) The rhizomes and the roots show little modification, except that the calibre of the vessels is generally smaller and the bark more precocious; (3) The ærial stems are shorter, more hairy, more spread out, closer to the soil and with shorter and less numerous internodes; (4) In general the stems have a cortical tissue that is less thick in proportion to the diameter of the central cylinder; the epidermal cells have thicker walls and the cuticle is more pronounced; often the epidermis is reinforced by a certain number of sub-epidermal layers; the different tissues of the central cylinder are ordinarily less differentiated; when bark exists, it appears earlier and is relatively thicker on branches of the same age; when there are secretory canals, they are relatively, or even absolutely, larger; finally, the stomata are more numerous; (5) Usually the leaves are smaller, except sometimes in sub-alpine regions, more hairy, thicker in proportion to their surface and often absolutely thicker, and deeper green by reflected or transmitted light; (6) The blade of the leaf acquires tissues better suited for assimilation; the palisade tissue is more strongly developed, either by a narrowing and elongation of its cells or by a considerable increase in the number of rows, the cells also contain a greater number of chlorophyll bodies and often each grain of chlorophyll has a greener tint. when there are secretory canals the diameter is relatively or absolutely greater; the epidermis of the leaf shows less differences than that of the stem, nevertheless, in general it is better developed, especially on persistent leaves, which have besides better developed protective sub-epidermal cells; the cells of the epidermis are ordinarily smaller and often the number of stomata per unit of surface is greater, especially on the upper face as M. Wagner was the first to show; (7) The petiole shows modifications generally analogous to those of the stems but much less pronounced; (8) The flowers are relatively much larger and sometimes even absolutely larger; they are more brightly colored and when the color is due to chromoleucites it is the same as in case of the chlorophyll grains, the number in a cell is greater, and often each chromoleucite is of a deeper color; the heightened color occurs also when it is due to substances dissolved in the cell sap. Experiments during eight years with *Teucrium* also show that modifications acquired

by the plant when it is taken from the plain to the mountain, or vice versa, disappear at the end of the same time when the plant is put back into its own climate. *Modification of functions*: (1) If a plant grown on the mountains is transported immediately to the level of one grown on the plains (both originally from the same root) we find for the same surface and under the same conditions, the chlorophyllian assimilation and the chlorovaporisation are more intense in the leaves brought from the alpine region; (2) If the respiration and the transpiration in the dark are compared in the same way, we find that for equal weights these functions have about the same intensity, or are less in the alpine specimens. The paper contains numerous wood cuts showing anatomical details and eleven lithographic plates comparing alpine and lowland individuals of the same species. The last is a double plate in color, illustrating the brighter hues of the mountain flowers. Foot notes refer to the principal literature.—ERWIN F. SMITH.

Spore Formation Controlled by External Conditions.—Einfluss der ausseren Bedingungen auf die Sporenbildung von Thamnidium elegans Link, by Johann Bachmann, is the title of the leading paper in *Botanische Zeitung* for July 16, 1895. *Thamnidium elegans* is a graceful little mould bearing two sorts of sporangia. The sporophore consists of a slender upright stalk, 2–4 cm. high and usually terminated by a single large sporangium, having a columella and bearing many spores. Midway down the sporophore there are usually one to ten or more whorls of branches which ramify dichotomously, often as many as ten times, the terminal divisions bearing singly on their ends small sporangia (sporangiola) generally only 6–8 μ in diameter and containing only a very few spores, usually 1–4. Sometimes only the end sporangium develops and sometimes only the dichotomous sporangioliferous branches; but the cause of this variation which is undoubtedly what led De Bary into the error of supposing Thamnidium a stage in the development of *Mucor*, has remained unknown. By varying his culture media Bachmann has discovered that he can at will produce sporophores with or without end sporangia and with or without sporongiola; in the same way he has been able to change the tiny sporangiola, which frequently bear only a single spore, into big sporangia provided with a columella and bearing many spores. As the result of his experiments he divides the fungus into six types as follows: (1) End sporangium present; sporangiola appearing very early on finely dichotomous branches which may reach the tenth subdivision, spores few. This form occured on more than a dozen differ-

ent media, the best results being obtained from the following: fresh, damp horse dung; dung decoction; agar-agar with 2½ per cent peptone; agar-agar with 4 per cent peptone and 0.5 per cent nitrate of potash. (2) End sporangia present; sporangiola 16–60 μ in diameter, with numerous spores and frequently with a columella and partial swelling up of the membrane. This type was obtained in nine different media, including the following: thoroughly cooked plums; damp bread; eggs; oranges; malt. (3) Only the end sporangium present. Obtained on slightly cooked plums and on 1 volume of malt extract in 2 vol. water. (4) Only the sporangiola present. Obtained in various culture media by raising the temperature to 27–30° C. (5) *a.* Mycelium with thick ends and gemmæ. This form was obtained in the following media: plum decoction with peptone; 1 vol. grape must in 4 vol. water with peptone; 1 vol. malt extract in ½ vol. water. *b.* Mycelium with fine ends and without gemmæ. Obtained in the following fluids: 1 per cent nitrate of potash with 1 per cent Nährlösung; almond oil with Nährlösung; oleic acid with Nährlösung; cane sugar in various percents. (6) Formation of zygospores. Not observed. According to the author, *Th. elegans* is the only fungus known which can be induced to form this or that sporangium, or none at all, by means of purely external, known conditions. He believes the production of the first type is due to substrata in which nitrogenous substances preponderate and fats and carbohydrates are present in only small qnantities, and that the second type is due to the reverse of these conditions. The paper contains 24 pages and is illustrated by a double plate.—Erwin F. Smith.

Germination of Refractory Spores.—In spite of every effort, it occasionally happens that the spores of a fungus refuse to germinate either in water or artificial media. This is true of various oospores, teleutospores and ascospores, and particularly and notably of the basidiospores of the whole group of the Gastromycetes, scarcely anything being known of the early stages of species of this group, owing to this fact. Recently, Dr. Jacob Eriksson, of Stockholm, has tried cold on a number of uredospores and æcidiospores with partial success. His method consists in placing the spores for several hours on blocks of ice or in a refrigerator at temperatures ranging down to minus 10° C. In a number of instances spores which refused to germinate in water at room temperatures, either wholly or in great part, did so freely and speedily after being on ice or in a refrigerator. In other cases the cold appeared harmful or without sensible influence, even on the same species. The opinion has been current for a long time that sudden great

changes in temperature favor the development of rust in cereals but usually this has been attributed to the indirect influence of cold in causing a deposit of dew in which the spores could germinate. In the light of these experiments this explanation can hardly be the true one. Spores which refused to germinate after lying in water several days germinated readily after exposure to cold. It would seem as if the cold were capable of stimulating the spores to germinate only when the latter have been rendered receptive by exposure to rainy weather, but further experiments and observations are necessary. It is at least certain that the spores of *Æcidium berberidis*, which germinated badly after cooling, were gathered in dry weather, while those which germinated abnndantly after cooling were gathered (on three different occasions) after several rainy days. The fungi tried by Dr. Eriksson were *Aecidium berberidis*, *Ae. rhamni*, *Ae. magelhænicum*, *Peridermium strobi*, *Uredo glumarum*, *U. alchemillæ*, *U. graminis* and *U. coronata*. The original paper, entitled Ueber die Förderung der Pilzsporenkeimung durch Kälte, may be consulted in *Centralblatt fur Bakteriologie und Parasitenkunde*, *Allg.*, Bd. I, p. 557.—Erwin F. Smith.

Botany at the British Association.—The presidential address of W. T. Thistleton Dyer before the new Section K (Botany) of the British Association at the Ipswich meeting (*Nature*, Sept. 26, 1895) is an exceedingly well written and interesting paper and one likely to obtain a wide reading. It deals with such topics as the following: Retrospect, Henslow, botanical teaching, museum arrangement, old school of natural history, modern school, nomenclature, publications, paleobotany, vegetable physiology, assimilation, and protoplasmic chemistry. The two and a half columns of sensible remarks on botanical nomenclature are specially commendable to American readers, as also what is said on teaching and in the last three topics of the address. It is certainly a surprise to learn that cramming for examinations from printed texts should be so largely taking the place of the careful study of plant phenomena in many English schools, the tendency in this country of recent years being happily in the other direction.—Erwin F. Smith.

Nitrifying Organisms.—Messrs. Burri and Stutzer, of the agricultural experimental station in Bonn, have discovered a bacillus (See *Centrb. f. Bak. u. Par. Allg.*, Bd. I, No. 20–21, 1895) capable of changing nitrites into nitrates and in many respects resembling Winogradsky's organism, but which *grows readily in bouillon and on gelatine.* This bacillus is much larger than the measurements given by Wino-

gradsky; like his it is incapable of converting salts of ammonia into nitrate, but unlike his is motile (when taken from colonies on gelatine or silicates), stains readily, causes slow liquefaction of gelatine, and is not yellowish but varies from colorless to bluish when grown on sillicates. The chemical activity is almost exactly the same as that of Winogradsky's bacillus and these authors, who have been studying the subject for two years, seem to think that it may after all turn out to be the same organism, the differences being less important than would seem at first sight, and resting perhaps on incomplete observations. The most important distinction appears to be the ability of this organism to grow on organic substances, but it does not appear from Winogradsky's publications whether he tried to transfer his organism from sillicate-plate cultures to bouillon, or gelatine, and failed.—Erwin F. Smith.

Relation of Sugars to the Growth of Bacteria.—Unquestionably the most discriminating and important paper that has yet appeared on this subject is a recent one, Ueber die Bedeutung des Zuckers in Kulturmedien für Bakterien (*Centrb. f. Bak. u. Par., Med.,* Bd. XVIII, No. 1), by Dr. Theobald Smith, now of Harvard. Reference is made to the literature of the subject but this is contradictory and many of Dr. Smith's interesting conclusions are largely or wholly the result of his own laborious and brilliant researches. The propositions are stated clearly and it is safe to say that hereafter no one will undertake the study of bacterial fermentation and gas production without first consulting this paper. The author's summary is as follows, but many things are not mentioned in this and the whole paper will repay the careful perusal of all who have groped about in this field of bacteriology: (1) In ordinary meat bouillon, souring and gas formation are only observed when sugar is present. Dextrose is the sugar most commonly attacked and muscle sugar is probably identical with it. (2) The formation of acid results from the breaking up of the sugar; the formation of alkali in the presence of oxygen results, on the contrary, from the multiplication of the bacteria themselves. So far as tested, the production of acid is common to all anærobic bacteria (facultative or obligate). (3) Facultative anærobiosis is made possible by the presence of sugar. (4) Rauschbrand and tetanus bacilli grew in fermentation tubes only when sugar was present. In test tubes containing the same sugar bouillon multiplication was never seen. (5) As far as tested, all gas-forming species produce along with CO_2 an explosive gas. (6) Souring as well as the production of gas are valuable diagnostic

characters, when at least three sorts of sugar are tested (with exclusion of muscle sugar). (7) Not only must the formation of gas be determined but also the progress of the same, the total quantity, and the quantity of CO_2. (8) For the differentiation of species and varieties it is of value to determine by titration the total amount of acid in 1 per cent sugar bouillon, as well as the germicidal power of such cultures on the bacteria themselves. (9) The division of bacteria into acid and alkali producers must be given up and the conditions govering the production of acid investigated more critically for each species. (10) The existence of fermentable carbohydrates in the digestive tract and in the fluids of the body is probably very favorable to the establishment and multiplication of pathogenic bacteria (both facultative anærobic and obligate, especially the latter).—Erwin F. Smith.

Algal Parasite on Coffee.—Under the title *Cephaleurus coffeæ*, eine neue parasitische Chroolepidee, Dr. F. A. F. C. Went describes in *Centrb. f. Bak. u. Par., Allg.*, Bd. I, No. 18–19, 1895, p. 681, an alga which he has found attacking the Liberian coffee at Kagok-Tegal in Java. This parasite appears on the leaves and berries in the form of round orange-brown spots which look bristly to the naked eye. The alga not only forms a thallus on the surface but sends its threads deep into the intercellular spaces of the host. The presence of the parasite in and on the leaf causes an interesting, protective hypertrophy of the surrounding tissue, the further progress of the alga being soon limited by a dense encircling mass of thick-walled, non-lacunose tissue, developed out of the palisade cells and spongy parenchyma of the leaf. No algal threads were found in this tissue. The berry not being able to defend itself in this way suffers most, becoming gradually brown and finally black and wrinkling and drying prematurely, so that the seed does not ripen. All parts of the alga are subject to the attacks of a fungus, which also appears to be capable of growing in the berries apart from the threads of the alga, but the relation of which to the latter and to the causation of the disease is left by the author in a rather unsatisfactory state. The paper is accompanied by a lithographic plate showing details of the alga and sections of the normal and hypertrophied tissue.—Erwin F. Smith.

ZOOLOGY.

On Bodo urinarius.—Although the discovery of certain peculiar infusoria in human urine dates so far back as 1859, but little is known of these animalculæ. M. Barrois has been investigating the subject

and has recently published his conclusions. According to his account Hassall was the first to detect this microscopic creature in its chosen habitat. He described it under the name of *Bodo urinarius*, as an animalcula $\frac{1}{1800}$ inch long and $\frac{1}{3000}$ inch wide, of rapid motion, generally round or oval, presenting a granular appearance, sometimes they are broader at one end. The long lashes, by means of which they move, are variable in number and proceed when there are two or three to each animalcule from opposite extremities; reproduction by longitudinal fission. In 1885 Kunstler found "small monads . . . . flagellate, transparent and very active . . . . probably *Bodo urinarius*."

In reviewing the subject, M. Barrois gives detailed accounts of these discoveries, and of the condition of the urine in which they appear. He then describes his own methods of investigation, and compares the drawings of specimens, after Hasslar and Kuntsler, with the infusoria he himself had found existing under similar conditions as those described by the authors mentioned. M. Barrois lays particular stress upon the fact that the infusoria found by him, only appeared in urine plainly alkaline, which contained animal matter (broken down epithelial cells, pus, albumen), and which had been exposed sometime to the air. In no case did he find them in fresh urine. Hassall's notes show a similar set of conditions in his case. Kunstler, however, claims to have found the infusoria in fresh urine in company with several species of bacteria. M. Barrois is of the opinion that Kunstler was deceived as to the age of the urine given him for examination, since in all other respects the conditions (as to animal matter, etc.) agree with those of Hassler and the author. In view of these conditions M. Barrois does not agree with the statement made that *Bodo urinarius* is a parasite. He is rather of the opinion that it exists in the air in a spore-like form ready to develop whenever it is brought in contact with a suitable nidus. This it finds in urine conditioned as above described.

In the course of his discussion, M. Barrois refers to *Trichomonas vaginalis* Douné, found by Salisbury in the urine and vaginal mucous of a young girl aged sixteen, supposed to be parasitic, and to certain Trichomonads found by Marchand and also by Miura; in all probability *T. vaginalis*. In the two latter cases, the infusoria was found living in freshly voided urine, so it would appear to be a true parasite. In both cases the urine was loaded with decomposing matter.

By an ingenious experiment, Miura demonstrated that the Trichomonads lived in the urethra only, and was not found in the bladder.

As to the classification of the Monads, M. Barrois considers it extremely unsatisfactory, since it is based on the number and disposition of

the flagella. In fact, *Bodo urinarius*, by reason of its polymorphism, can have no place in such a scheme of classification.

In conclusion, the author compares *Bodo urinarius* with *Oekomonas mutabilis* Saville-Kent, which propagates both from spores and by fission in infusions of vegetable matter, and also with *O. rostratum* Sav.-Kent, found in both fresh and salt water containing vegetable debris. He finds the three species so similar in appearance, that one might infer that their only difference is in their habitat.

M. Barrois repeats, as a final statement, that *Bodo urinarius* Kunstler (=*Cystomonas urinaria* R. Bl.=*Plagiomonas urinaria* M. Braun) can hardly be given a place among the parasites of man. (Revue Biol., Feb., 1895.)

Influence of the Winter 1894-1895 upon the Marine Fauna of the Coast of France.—M. Pierre Fauvel calls attention to the considerable influence which the exceptional lowering of temperature, and long duration of cold, during the last winter, exercised upon the marine fauna of the coasts of France.

Sharp frosts, at the time of high tide, would destroy innumerable quantities of animals that the ebb tide would leave exposed. Annelids, Actinans and Fish were found dead or unconscious, paralyzed by the cold. This mortality, strange to say, extended to depths which the change of temperature could not have affected directly.

Another effect of the cold has been to bring in shore animals ordinarily seen in deeper water, and also certain species very rare or entirely unknown in our fauna. The Spring was marked by an extraordinary abundance of *Balanus porcatus*, which covered with a continuous bed the surface of the boulders and rocks, and by the return of the Mussels which had nearly disappeared. During some weeks *Mytilus edulis* took possession of all the rocks exposed to the southwest wind and formed veritable "moulières" at Dent, Pointe de Réville and at Draguet. Parallel changes are noticed in the annelid fauna. Thus certain species which were common last year have either become rare, or totally extinct, while new species are continually taking their places. (Revue Scientif., 1895, p. 374.)

Preliminary Outline of a New Classification of the Family Muricidæ. By F. C. Baker (Bull. Chicago Acad. Sciences, 1895). On reading this paper we regret to find that Mr. Baker has been putting his new wine into old bottles. In other words, he has borrowed largely from the phraseology of a conchological paper published in 1892, as the following parallel passages show :

Pilsbry, 1892.

"For several years the writer has been accumulating data bearing upon the natural classification of the Helicoid land snails. It has been thought desirable to place before students of this group some of the general results attained, and to invite their friendly criticism.

" * * * the author's aim being simply to place before malacologists the outlines of a classification essentially modern and essentially original."[1]

Baker, 1895.

"For several years the writer has been accumulating data bearing upon the natural classification of the Gastropod family Muricidæ. It has been thought desirable to place before students some of the results elucidated, and to invite their friendly criticism.

"The author's aim in the present paper has been simply to place before malacologists the outline of a classification essentially modern and essentially original."

[1] The above quotation is from Pilsbry's Preliminary Outline of a New Classification of the Helices, Proc. Acad. Nat. Sci., Phila., 1892, p. 387. Good taste should have forbidden the reproduction by Mr. B. of the second paragraph here quoted, the egotism of which is excusable only in view of its undeniable truth in relation to the 1892 publication. This excuse seems to be lacking in the case of Mr. Baker's paper.

More to the same effect might be quoted, but the above is sufficient on this score.

We do not wish to imply that there is any great harm in using borrowed phrases; they are not copyrighted, and their original author probably does not expect to make use of the same sentences again; but, still, if anybody has ideas worth expression, they surely ought to be worthy of fresh verbiage.

In regard to Baker's subfamilies, we do not see that they differ from those of Tryon and Fischer, except that Baker includes *Coralliophila* and its allies as a third subfamily. As this group lacks teeth, it seems much better to treat it as a family. In this connection it may be well to state that *Latiaxis mawæ* is not a monstrosity as Baker's foot-note (p. 188) would seem to imply.

The diagnoses of subfamilies given are rather absurd in view of their contents, which contradict every word of the descriptions. Not all the genera placed in "*Muricinæ*" have spinous or foliated varices, not all have the nucleus of operculum apical, and not all have few cusps on the rhachidian teeth. What is the use, then, of such a "subfamily?" Among the genera we notice, on a cursory inspection, that *Murex tenuispina* Lamarck is quoted as type of *Murex* Linné. How can

Lamarck's species, published a half century *later* than Linnæus' genus, be the type of that genus? The type of *Pterorhytis* Conrad ("Pterohytis" Baker) is not *Ocinebra nuttalli* Conr. but *Murex umbrifer.* Other mistakes of this nature occur, but we have not space to notice more.

The citation of the pre-Linnæan "genera" of Klein is contrary to all codes of nomenclature recognized by modern zoologists, and the continuation of such anomalies is to be deprecated. In retaining *Tribulus, Pentadactylus*, etc., as of Klein, Mr. Baker is clearly in error.

Most, if not all of the innovations in nomenclature proposed in this paper, are borrowed from Fischer and Dall. We find no new facts in regard to either soft anatomy or shell structure in the entire article, so that Mr. Baker's claims for originalty and modernness do not seem sufficiently apparent to call for special remark.—H. A. PILSBRY.

Herpetology of Angola.—The Herpetology of the Portugese possession in Western Africa, just published by Barboza du Bocage at Lisbon comprises descriptions of 185 species, distributed as follows; Chelonia 10, Loricata 3, Sauria 57, Ophidia 74, Batrachia 41. Of the specimens described, 62 species and varieties belong exclusively to the fauna of Angola and Congo. In order to better appreciate the relation which the herpetological fauna of these two areas bears to that of the rest of Africa, a table of the geographical distribution of the species described is given and forms an important adjunct to the paper. A number of new species are described, and synonymy is corrected. The paper is handsomely illustrated, and forms an important contribution to the knowledge of the subject.

Among the points of interest embraced in the paper are the discovery of the new species: *Naja anchietæ, Dendraspis neglectus, Vipera heraldica* and *Python anchietæ;* the southern range of the West African *Osteolæmus tetraspes, Feylinia currorii, Atheri squamigera*, and *Hylambates aubryi;* the northern range of the South African *Mancus macrolepis, Zonurus cordylus*, etc. and westward range of the central African *Causus resimus.*

Zoological News. ;BIRDS.—In regard to the question of the value of the forms of the tongues of birds for classification, Mr. F. A. Lucas concludes that in the Woodpeckers the evidence favors the view that the modifications of the tongue are directly related to the character of the food, and are not of value for classification. (Bull. No. 7. Div. Ornith. and Mam. U. S. Dept. Agric., 1895.)

In the study of the hyoid bone of certain parrots, Mr. Mivart finds that the whole order of Psittaci is distinguished from every other order of birds by the shape of its hyoid. The distinctive characters are (1) Basihyal much broadened posteriorly. (2) Basihyal developing on either side a forwardly and upwardly directed process. (3) An *os entoglossum* in the form of a single broad bone with a considerable central foramen, or, in the form of two lateral parts, entoglossals, medianly united in front by cartilage and leaving a vacant space between this and their attachment behind to the basihyal. (Proceeds. Zool. Soc. London, 1895, p. 162.)

MAMMALS.—Mr. Outram Bangs distinguishes the Skunks of eastern North America as follows:

Mephitis mephitica (Shaw), ranging through the Hudsonian and Canadian zones of the east, south to about Massachusetts.

Mephitis mephitica elongata (Bangs), found in Florida and the southern Atlantic states and ranges north to about Connecticut.

Both of these species differ from the western skunks, which form a separate group.

Among the latter the author recognizes Richardson's *Mephitis americana* var. *hudsonica* as a good species which must therefore bear the name *M. hudsonica* (Richardson). It is the largest of all the skunks, and has an extensive range in the northern prairies, extending east as far as Minnesota. (Proceeds. Boston Soc. Nat. Hist., Vol. XXVI.)

ENTOMOLOGY.[1]

Insects in the National Museum.—The staff of the Department of Insects of the U.S. National Museum has been reorganized, as a result of the sad death of the former Honorary Curator, Professor C. V. Riley.

The reorganization has been effected by the appointment of Mr. L. O. Howard, Entomologist of the U. S. Department of Agriculture, to the position of Honorary Curator to the Department of Insects; of Mr. Wm. H. Ashmead to the position of Custodian of Hymenoptera, and Mr. D. W. Coquillett to the position of Custodian of Diptera. All museum custodians are honorary officers. Mr. M. L. Linell will remain as general assistant to the Honorary Curator.

The Department is, at present, in excellent working condition. It contains a very great amount of material in all orders, and, in many

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

unusual directions, surpasses any collection in the country. Among others, the following are of especial interest:

(1) The large collection, in all orders, of the late Dr. C. V. Riley.

(2) All of the material gathered during the past 18 years by correspondents, field agents, and the office staff of the Division of Entomology, U. S. Department of Agriculture.

(3) The greater part of the collection of the late Asa Fitch.

(4) The large collection, in all orders, of the late G. W. Belfrage.

(5) The collections in Lepidoptera and Coleoptera made by Dr. John B. Smith down to 1889, together with the types of the Noctuidæ. since described by Dr. Smith.

(6) The collection of the Lepidoptera of the late O. Neske.

(7) The collection of Lepidoptera of G. Beyer.

(8) The collection of Coleoptera of M. L. Linell.

(9) The bulk of the collection, in all orders, of the late H. K. Morrison.

(10) The collection of Diptera of the late Edward Burgess.

(11) The type collection of Syrphidæ made by Dr. S. W. Williston.

(12) The collection of Ixodidæ of the late Dr. George Marx.

(13) The collection of Myriopoda of the late C. H. Bollman.

(14) Sets of the neo-tropical collections of Herbert Smith.

(15) The collection of Hymenoptera of Wm. J. Fox.

(16) The collection of Tineina of Wm. Beutenmuller.

(17) The large Japanese collection, in all orders, of Dr. K. Mitsukuri.

(18) The African collections, in all orders, of Dr. W. S. Abbott, Wm. Astor Chanler, J. F. Brady, the last "Eclipse" expedition to West Africa, and of several missionaries.

(19) The large collection from South California of D. W. Coquillett, in Coleoptera, Hymenoptera, Lepidoptera and Orthoptera.

(20) The Townend Glover manuscripts and plates.

In addition to this material, there are minor collections which have been the result of the work of government expeditions, or are gifts from United States Consuls and many private individuals.

This enormous mass of material is being cared for by the active and honorary forces of the Department, and the perpetuity of the collection is assured. The National Museum building is fireproof, and this, together with the fact that it is a national institution, renders the Department of Insects perhaps the best place in this country for the permanent deposits of types by working specialists in entomology, and for the ultimate resting-place of large collections made by individuals.

The policy of the Museum at large, with regard to the use of its collections by students, is a broad and liberal one. Students are welcome in all departments, and every facility is given to systematists of recognized standing.

On the Girdling of Elm Twigs by the Larvæ of Orgyia leucostigma, and its Results.[2]—The white-marked tussock-moth *Orgyia leucostigma*, has, for a long term of years, been exceedingly destructive to the foliage of the elms, horse-chestnut and fruit trees in Albany. Fruit trees of considerable size have been killed by their defoliation within a few days, toward the maturity of the caterpillar. Large elms and horse-chestnuts have had the foliage entirely consumed, only the ribs and principal veins remaining.

In the summer of 1883, a new form of attack by this insect was observed by me in Albany. About the middle of June of that year, the sidewalks, streets and public parks where the white elm, *Ulmus americana* was growing, were seen to be thickly strewn with the tips of elms two to three inches in length, bearing from four to ten fresh leaves, and comprising nearly all of the new growth of the season. On examination, it was found that above the point where the tips had been broken off, the bark had been removed for an extent averaging about $\frac{1}{10}$ of an inch, apparently by an insect.

As the Orgyia larvæ were then occurring in abundance on the trees, they were suspected as being the authors of this injury, and the suspicion was verified by ascending to a house-top, where the roof was found to be heaped in the corners with the severed tips, and the caterpillars engaged upon the branches in the girdling. The explanation of the breaking-off was simple. With the removal of the bark, the decorticated portion—not exceeding, in many instances, in thickness the diameter of a large pin—dried, and becoming brittle, was readily broken by a moderate swaying of the wind.

The girdling of the twigs in this manner could serve the Orgyia no such purpose as attends the girdling of several other insects, as the *Elaphidion* pruners of oaks and maples, where it enables the insect to attain greater security for its transformations through this method of reaching the ground, or the *Oncideres* twig-girdler, where the dead wood affords suitable food for the larva. Probably the conditions of growth during the spring of this year were such as to render the young bark at the point attacked particularly attractive to the larvæ; but why, after feeding upon it to so limited an extent, it should cease and resume its feeding on the leaves, can not be explained. In a few in-

[2] Read before the American Association for the Advancement of Science, at its Springfield meeting, Sept. 3, 1895.

stances where the twigs had become detached quite near the node marking the commencement of the year's growth, the bark had been irregularly eaten for an inch or more in extent.

While the Orgyia is a serious pest in Albany, it has its years of remarkable abundance and of comparative scarcity. Girdled tips, as above described, have been seen each year since 1883, but by no means corresponding in number to the degree of abundance of the caterpillar. My attention had not been drawn to them the present year, until much later than the usual time—toward the end of August. At this time (21st of August), many tips of unusual length and with perfectly fresh leaves were collected from beneath a large American elm. Each one had broken at the base of the girdling, which had probably been quite near the node of the year's growth. They were of special interest from their great length, varying from 10 to 18 inches. From the growth they had attained, it was evident that the girdling had not been done in the spring or early summer, but in the late summer after the usual brood had completed its transformations. It was clearly the work of a second brood of the insect, and this was confirmed by my having seen, a few days previously from a house-top, while making observations on the elm-leaf beetle, the Orgyia larva about one-half grown.

A distinct second brood of the Orgyia has not been recorded in Albany, although it is known to be double-brooded in Washington and Philadelphia, and probably in Brooklyn, and has also been observed in Boston. The present year, however, has been an exceptional one in the remarkable abundance, the rapid development, and the injuriousness of several of our more common insect pests.

Another interesting feature connected with these tips was the illustration they gave of the manner in which woody structure is built up —the sap ascending through the sap-wood, and, after its assimilation in the leaves, returning through the inner bark and depositing its organized material. The bark above the girdling, in healing in a rough and irregular manner, had swollen out at this point in a bulbous-like enlargement, showing very clearly the arrest and deposit of the returning sap consequent on the absence of its natural channels, and the drying and the death of the decorticated wood below it. In a specimen gathered in which the node of the preceeding year remained attached to the fallen twig, the diameter of the new growth above the bulb was at least twice that of the starved node below.

This peculiar form of Orgyia attack has not been seen upon the horse-chestnut, maple, apple or plum, or any of its other food-plants.

J. A. Lintner.

Albany, N. Y.

EMBRYOLOGY.[1]

Experimental Embryology.——Recent numbers of Roux's Archiv für Entwickelungsmechanik contain numerous additions to our knowledge of the possibilities resident in the early stages of the development of animals, possibilities unsuspected till direct experimental interference made them evident.

T. H. Morgan of Bryn Maur presents evidence[2] to show that two blastulæ of the sea urchin, Sphærechinus, may fuse together and form one embryo. When eggs are shaken just after fertilization they may loose their membranes and afterwards some of the resulting blastulæ are found to have twice the normal size though otherwise like the usual blastulæ in appearance. Such large blastulæ are stated to arise from the fusion of two common blastulæ.

Notwithstanding this complete fusion the future development of such enlarged blastulæ gives evidence of their dual origin. At the gastrula stage *two* invaginations are formed.

One may be much the greater and the two may not appear at the same time. The two invaginations stand in no fixed relation to one another and may appear in all parts of the compounded blastula.

Later the larva that develops from two fused blastulæ tends to develop two sets of arms and two systems of skeletal rods, but those accompanying the lesser invagination are much reduced in size and less perfect than the rods associated with the main invagination.

A second paper[3] by the same worker records a variation in the cleavage of the above sea urchin when some of the eggs were shaken.

While most of the eggs divide into 2, 4, 8 and 16 cells some were found to divide at once into *three*. These 3 cells are elongated parallel to the planes that produced them. When they next divide they all do so lengthwise, in flat contradiction to "Hertwig's law." These six equal cells lie in a plane at right angles to the two cleavage planes that have produced them.

Such eggs may develop into gastrulæ. They form six small cells or micromeres at one pole of the mass in place of the normal four. The author thinks "a micromere field must have been present in the egg prior to division."

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts, reviews and preliminary notes may be sent.

[2] Vol. II, pps. 65–70.

[3] Idem, pps. 72–80.

Eggs that have not been shaken sometimes divide at once into four cells.

In both these unusual forms of cleavage the author finds that the three or the four archoplasmic centres present in the egg take unequal numbers of chromosomes. Thus in one case one centre was accompanied by 17, another by 14, another by 33 and the fourth by 0 chromosomes.

That this inequality is greater in the four-fold than in the three-fold division explains, the author suggests, the fact that fewer eggs develop from the four-fold than from the three-fold cleavage.

A third paper[4] gives a detailed account of the partial larvæ obtained when the eggs of Sphærechinus are shaken into fragments. Very minute gastrulæ only $\frac{1}{64}$ part of the volume of a normal gastrula are thought to come from isolated pieces with $\frac{1}{40}$ to $\frac{1}{30}$ the volume of the whole egg.

It is found that the number of cells in such small blastulæ is less than the normal number and roughly proportional to the size of the blastula.

The size of the nuclei, and probably of the cells also, is less in the small blastulæ than in the normal ones.

If one of first two cells of a cleaving egg be isolated it may form a blastula with $\frac{1}{2}$ the normal number of cells. One of the first four cells gives a blastula with $\frac{1}{4}$ the normal number, or with a little more than $\frac{1}{4}$; while one of the first eight cells when isolated produces a blastula with more than $\frac{1}{8}$ the normal number.

Such blastulæ will develop into gastrulæ.

A piece of the wall of a blastula when broken off by shaking may develop into a gastrula.

The little blastulæ formed from fragments of eggs tend to invaginate as many cells as possible up to the normal number for a normal gastrula.

These remarkable numerical relations lead the author to suppose that the reason why isolated cells of later stages in cleavage are not able to develop by themselves lies not in any differentiation of nuclear substance but in the fact that such cells being themselves the results of a series of cleavages cannot produce cells enough for the next stages of development.

Morgan and Driesch publish conjointly[5] their reinvestigation of the remarkable halflarvæ obtained by Chun.

[4] Idem, pps. 81–124.

[5] Idem, pps. 203-226.

Chun's work was done 18 years ago and was, as stated in a letter to Roux ;[6] as follows.

When the first two cells of the eggs of the lobate Ctenophore, *Bolina hydatina* were separated by shaking each developed as a halflarva with four ribs or bands of locomoter appendages instead of the normal eight, two entodermal sacs in place of four and only one tentacle in place of two.

The first cleavage plane coincides with the sagittal plane of the adult and the second with the transverse.

Half-larvæ with 4 ribs, 4 meridional vessels one tentacle and an oblique stomach *may become sexually mature,* developing eggs and sperm under the two subventral meridional vessels!

The missing half is regenerated during the postembryonic metamorphosis.

Driesch and Morgan worked on another Ctenophore, a nontentaculated form, *Beroë ovata* and finding it impossible to employ the shaking method cut the eggs with special scissors.

Isolated cells of the two cell stage develop into blastulæ, gastrulæ and finally into larvæ that are most remarkable in being neither complete nor halflarvæ but larvæ deficient in certain organs.

The cleavage of such an isolated cell is much as it would be if still associated with the other cell in a normal egg : it is a half cleavage as compared with a normal egg. This, however, is not true of the cells that form the ectoderm but only of the peculiar group of cells forming the entoderm. The former cells grow over the half-group of entoderm cells and form a larva that is complete on the surface.

The final larva is abnormal in usually having only 4 ribs instead of 8 and 3 pouches instead of 4.

A second series of experiments seems to throw much light upon the influence of protoplasm versus nucleus in the causation of such imperfect development.

When a piece of the protoplasm of an entire egg is cut off, the egg, deprived of some protoplasm but with it nucleus intact, as far as known, develops into a larva that may be deficient in just the same way as is a larva reared from one of the isolated cells.

In another paper[7] Morgan finds the shaking method will not succeed with the blastulæ of Sphærechinus as they die when shaken. In Echinus, however, both blastulæ and gastrulæ may be shaken into pieces that will live.

[6] Idem, Oct. 25.95, pps. 444–447.

[7] Idem, pps. 257–266.

When pieces of the wall of the Echinus blastula are broken off they may form little blastulæ again and these may gastrulate. When these little blastulæ invaginate they tend to form more entoderm cells, in proportion to the entire number, than is the case in the normal blastula.

In Sphærechinus the normal blastula has about 500 cells and tends to invaginate about 50; in Echinus about the *same fraction of the whole* is invaginated, for of about 1000 cells about 100 go in to form the entoderm.

When young gastrulæ are shaken they may form abnormal larvæ owing, apparently, to changes in the mesoderm inducing abnormal skeletal growths and corresponding abnormal arms.

Pieces shaken out of the wall of a gastrula will not form into a blastula nor into a gastrula.

Likewise the entoderm when shaken out does not develop. Yet a gastrula that has had its entoderm removed by shaking will continue to grow and form a normal skeleton and arms.

A paper[8] on cross fertilization and the fertilization of non-nucleated pieces of eggs also by MORGAN goes over part of the ground of Boveri's remarkable work.

It is shown that small pieces of eggs of *Echinus miliaris* may be fertilized and develop as far as to the 16 cell stage. As the number of chromosomes in such cleaving masses is, in each nucleus, half the normal number it is inferred that such cleaving masses are the results of the entrance of one spermatozoa into a non-nucleated piece of an egg.

In attempting to cross fertilize pieces of eggs of Sphærechinus with sperm of Echinus it was found that the sperm entered the pieces only in few cases; there is the same difficulty in crossing pieces of eggs as in crossing the whole egg with foreign sperm. The reverse is also the case; sperm of Sphærechinus will not readily enter pieces of eggs of Echinus.

It is, therefore not surprising that no larvæ were found that could be traced to non-nucleated pieces of eggs fertilized by a sperm of another species, which is the great desideratum in attempting to repeat Boveri's work.

When the whole eggs of Sphærechinus are fertilized by sperm of Echinus bastards result that are very variable and not all exact middle states between the larvæ of these two species. When the converse cross is attempted the larvæ are "for the most part very abnormal in appearance."

When the eggs of Sphærechinus are crossed with the sperm of Strongylocentratus the larva is very variable and not an intermediate form.

[8] Idem, pps. 268–280.

The converse bastards also show great variation in the skeleton.

BOVERI[9] republishes, is an amplified form with many new illustrations, his remarkable work on the cross-fertilization of enucleated fragments of sea urchin eggs, translated in the AMERICAN NATURALIST March 1, 1893. After considering the opposing results obtained by Morgan and by Seeliger the author still maintains that he has shown that a larva may be obtained from a piece of an egg without any nucleus and sperm of another species and that such a larva has none of the maternal characters but only those of the male parent, thus showing that the nucleus may transmit characters but that egg cytoplasm alone cannot do so.

The evidence for his conclusions is, however, of an inferential nature and a cautious jury may well hesitate before convicting Boveri of having deprived the cytoplasm of its share in the affairs of heredity.

The evidence as he now presents it seems to be about as follows.

1. When at Naples in 1889 he shook a lot of sea urchin eggs, *Echinus microtuberculatus* in a testtube many were broken into pieces of various sizes, with or without nuclei; when this collection was treated with sperm of the same species larvæ of all sizes down to $\frac{1}{20}$ of the normal were found.

2. When pieces that contain no nucleus, as far as could be seen under the microscope, were isolated and fertilized with sperm of the same species they developed into dwarf larvæ.

3. When the normal eggs of *Sphrærchinus granularis* are treated with the sperm of *Echinus microtuberculatus* some few bastards result. In Boveri's original experiment all these bastards were half way between the larvæ of the parent species, both in external form and in the skeleton, which were very different in each pure larvæ form.

4. When the eggs of *E. microtuberculatus* are shaken and treated with sperm of the same species the larvæ present many abnormalities and some may have characters resembling those of another species.

5. When shaken eggs of *S. granularis* are treated with sperm of *E. microtuberculatus* large and small larvæ are formed; some few of the small ones, only a very few, 10 or 12 in all, were entirely of the Echinus or father type.

6. It was observed that the nuclei in any given area of a larva formed from a nonnucleated piece of Echinus egg and the sperm of the same species were, *on the average*, smaller than the nuclei in the corresponding area of a smaller larva formed from a nucleated piece.

7. The above few bastard larvæ of pure Echinus type had, on the average, smaller nuclei than similar larvæ of type intermediate between the two crossed parent.

[9] Idem, Oct. 22, 1895, pps. 394–441.

The author maintains that the few dwarf bastards that were like the male parent came from nonnucleated pieces of Sphærechinus penetrated by a sperm of Echinus. This, however, is an indirect result of all the above facts, It should also be borne in mind that Morgan, as cited above, was not successful in obtaining a cross fertilization of enucleated fragments and that both Morgan and Seeliger[10] find the bastards between Sphærechinus and Echinus so variable that Boveri's experiment in which they appeared as exact intermediate forms seem an exception and hence may be withdrawn from the evidence.

Hans Driesch starting from the standpoint that it has been proved that isolated cleavage cells may produce an entire organism seeks to find where the limits of this power appear in the subsequent stages of development.

He cut blastulæ of Sphærechinus into pieces, chopping at random with scissors in a dish full of gastrulæ. When isolated the larger pieces formed gastrulæ or later larvæ, in most cases.

When the gastrulæ of the starfish, *Asterias glacialis* were cut in the same way some lost the inner end of the gastric invagination together with much ectoderm. After healing over the wound the new end of the gastric invagination enlarged and sprouted out the two cœlomic pouches that would have, normally, been formed from the part that was cut away: the power to form cœlemic pouches was thus vicariously assumed by a part that would normally have produced part of the definitive digestive tract. Such larvæ go on to form a normally three chambered digestive tract from what would, normally, have formed but part of the whole.

The author concludes that the powers of the ectoderm or of the entoderm cells are as yet not restricted as to what organs or parts they may form in their proper germ layer.

When larvæ with the mesenchyme were cut and a piece with only ectodermal cells was isolated in 53 cut of 99 cases no gastrulation took place but only a healing of the wound though life and activity might last for a week.

In a few cases when the digestive tube was removed from such larvæ it did not grow but died after a few days. In 19 cases where the end of the gastric invagination was removed after it had enlarged and sprouted out the cœlemic pouches 17 did not form new cœlemic pouches. In like manner when a cut happened to remove the skeleton of one side it was not formed again.

We thus soon come to a state in which the primitive tendency of cells to replace others in organ formation seems lost.

[10] See American Naturalist, March, 1895.

In all these cases the author will not grant that any *true regeneration* of lost parts takes place.

In the course of his discussion of the influence of yolk upon gastrulation Paul Samassa[11] states that he has performed the following experiments upon frogs' eggs.

The eggs were injured, without breaking the egg membrane, by an induction shock applied from needle points to certain cells or groups of cells in the eight-cell stage of cleavage.

When the four vegetative cells are injured there may result a cell mass lying in two layers upon a mass of yolk and evidently representing, as seen in actions, a normal gastrula minus parts of its ectodermal structures. On the other hand injury to the animal cells results in a mass of cells lying near an inert mass of matter and possibly representing only entodermal cells.

These facts are interpreted as meaning that the animal or the vegetative cells may continue their development for some time independently of life, or at least of the perfect state, of the other cells.

These eggs die without reforming the injured parts by postgeneration.

In an extensive theoretical part of the paper the author places himself in the main, on the side of Hertwig as believing in epigenesis rather than in any form of preformation.

The last paper that we can notice here is that of Amedeo Herlizka[12] who succeeded in tying a thread about the eggs of *Triton cristatus* in such a way as to completely separate the first two cleavage cells.

Hertwig compressed eggs by this method so that they formed hourglass shaped masses a single embryo finally resulted.

But in these experiments where the egg is separated into two distinct halves each develops by itself. From one half of the egg there results an embryo that may live to have a medullary tube and a notochord. This embryo was formed by a process of cleavage like that of an entire egg and not like the cleavage of half an egg.

The author holds that neither Weismann's nor any other ideas of preformation will suffice to explain such phenomena, but that we must accept some form of epigenesis.

He thinks that each of the first two cells is *totipotent* and normally makes half of the embryo when in the normal union with its fellow because of some *inhibition* of its power to produce the whole. In a case of postgeneration we might suppose that this inhibition was removed for a while and then resumed.

[11] Idem, Oct. 22, 1895.

[12] Idem, pps. 352–366.

ANTHROPOLOGY.[1]

Discoveries at Caddington, England, by Mr. Worthington G. Smith.—M. Renach informed the writer at the St. Germain Museum, in 1893, that a hermit was needed in France to live in the Drift Gravel Quarries and pounce upon chipped blades as they were brought to light in the excavations. This was to illustrate the fact that about four-fifths of the alleged paleolithic implements on exhibition in France were either found on the surface and not in place in the gravels, or bought by collectors, professors of geology and curators of museums, as I bought mine from workmen at the gravel pits.

Nevertheless, there is sufficient evidence of a Plistocene blade chipper in western Europe to satisfy the American critic who will take nothing on faith, and the best of this in recent years is embodied in the work of Mr. F. G. Spurrell, who found a stone blade workshop of Plistocene age under drift gravel at Crayford, England, and in the indefatigable explorations of Mr. W. G. Smith at North London (Stoke Newington) and at Caddington, Bedfordshire.

"Man, the Primeval Savage," by Worthington G. Smith, London (Edward Stanford, 27 Cockspur St., Charing Cross, 1894), tells of the striking discoveries made by the latter in some brick-kiln pits on a hill top near Caddington. These cuttings through the drift, discovered by tracing up relic bearing road ballast to its source, and watched for six years, during which time they were often filled with water or abandoned by workmen at critical moments, revealed what Mr. Smith calls a Paleolithic floor or older surface on which rested a stone blade workshop of Plistocene Age. This was covered by a mantle five to ten feet thick, of contorted drift, unfortunately containing no animal remains, that here overspreads the hill, and developed upon examination the following interesting and novel facts:

1. The blade factory was undisturbed, thus presenting an association of artifical objects full of significance and duplicating the results of Mr. Spurrell at Crayford. Other discoverers had found scattered and isolated specimens in the gravel, here the raw material, the blades more or less finished, the chips and the tools lay just as the Post-Glacial workmen had left them.

2. To the envy of the ordinary searcher for isolated objects in the drift, this range of specimens from one place included scrapers worked

[1] The department is edited by Henry C. Mercer, University of Penna., Phila.

on one side, well specialized leaf-shaped blades, either worked all round or sharpened to points, "punches," knife-shaped blades, hammer stones, "anvils," flaked cores and nodules worked in an exceptional way.

3. Discovered blocks of raw material, flint nodules with chalk still adhering to them, showing that the workmen had pulled them out of neighboring flint bearing chalk beds, lay in piles at the site.

4. Several large nodules had been sharpened at one end, leaving the rest of the nodular surface untouched.

5. The hammer stones found were not the numerous oval flint pebbles lying about the site and showing no signs of pounding (though they had been brought to the spot by workmen), but less regular fragments of flint, sometimes knocked into shape and scored with the marks of battering. Sometimes they weighed from five to six pounds.

6. Large flint masses, called by Mr. Smith "anvil stones," were found, showing slight traces of bruising, which, owing to slight doubts of the explorer, were not preserved.

7. The punches discovered were thin, stalactite-shaped nodules, bruised at both ends, weighing sometimes a pound or more, which with "fabricators," pieces of nicked flint used for flaking, in the explorer's opinion, were found mixed with the blade refuse. As opposed to Mr. Smith's view of flaking by means of stone punches and "fabricators," we know that the North American Indians, when working under similar circumstances, used bone, though a relic forger showed the explorer how the Caddington specimens could be accurately reproduced with an iron hammer and a broken gimlet or awl used as a punch.

8. Cores were discovered from which flakes had been worked (a) by careful blows, (b) by smashing with heavy blocks.

9. A beautifully veined pebble, found at the spot, had been brought there as an object of value by the ancient blade workers.

10. Several piles of apparently selected flakes were discovered.

11. A twin flake, held together by a fine, unsplit section, ready to break at a slight jar, was found with the refuse, showing that the workshop site, an area probably covering nearly an acre, had been very gently overspread with the now overlying drift-material, a deposition which had failed to seriously disturb the situation. Mr. Smith, who was present at the brick-pits, at short intervals, for nearly six years, in gathering this remarkable evidence, repeated observations previously made by him at Stoke Newington, Common, London, where, besides duplicates of many of the specimens referred to above, he found two artifically pointed stakes, a scratched log and a chipped blade resting on the scapula of a Mammoth (now on exhibition at the British Mu-

seum). At another place near Caddington, he had found associated with drift blades and in place a horde of two hundred of the bead-like fossils (*Cocinopora globularis*), with holes artificially enlarged, though at none of the sites were drawings on bone, bone needles or lance heads discovered. One of the most interesting features of the work at Caddington consists in what Mr. Smith calls "replacement," a process previously invented by Mr. F. G. Spurrell, and never before, to my knowledge, applied to drift specimens found in situ.

The two thousand two hundred and fifty-nine flakes unearthed at Caddington were grouped according to color on small trays easily shifted from table to table, and a laborious experimental study of them, lasting for three years, demonstrated the interesting fact that many sets of them fitted together, sometimes reconstructing the original nodule on which the blade maker had worked, sometimes hedging about hollows which, on pouring in plaster of Paris, reproduced the form of the resultant and missing blade.

"I examined and re-examined the stones," says Mr. Smith, "almost daily. I looked at them as a relief from other work and at times when I was tired.

"Not only did I keep my selected stones on the tables for this length of time, but I kept a vast number of blocks, rude pieces and flakes, on certain undisturbed grassy places in the brick-fields for the same three years. Whilst working upon my tables, I sometimes suddenly remembered one or more like examples on the grass, and at an early opportunity, fetched them from Caddington. In making up some of the blocks of conjoined flakes, it often happened that one or more interior pieces would be missing. In some cases, these missing pieces were never found, but in other instances, after the lapse of months, or even more than a year, a missing piece would come to light on the paleolithic floor. It is certain that I have not replaced all the flakes in my collection that are capable of replacement—one reason for this is that many flakes are very different in color and markings on one side from what they are on the other, and it is difficult to remember the markings on both sides. Another reason is that the time at my disposal has not been *unlimited.*"

All this demonstrates in a manner, as conclusive as it is novel, that the Caddington site is an *undisturbed* workshop, while the analyses of Mr. Smith and the facts described in his work—Man, the Primæval Savage—take precedence over all recent evidence upon the subject, and throw a new light upon the more ancient subdivision of the Stone Age in Europe.

He who has spent earnest hours upon the problems of Plistocene humanity would gladly have seen a department of a museum specially devoted to these unique discoveries and demonstrations, but in a visit to Caddington in 1894, I learned with regret that the series, highly important from its entirety, and not jealously guarded as a whole, had been dissipated for the sake of collectors who wished to illustrate certain phases of Paleolithic blade manufacture with "fine specimens."

Theory, and with it the desire to propound formulæ for the blade-making process in general, yield respectfully to these toilsome investigations and to the persistent ransacking of quarries by a faithful observer whose work alone answers many of the doubts of the American student, and counteracts the questionable impression left upon the mind of the visitors to European museums by rows of typical specimens bought from workmen or gathered upon the surface.

H. C. Mercer.

Recent Explorations of Captain Theobert Maler in Yucatan.—[Extract from a letter received by the editor, December 9th, 1895].—After your departure from Yucatan, I undertook an expedition to the *Peten' Itza* region (Guatemala), crossing the entire pensinula, whose interior or southern part is nearly unknown.

After examining the country around the great Laguna of *Peten' Itza*, I embarked on a small canoe on the Rio Dela Pasión (" which, farther down, is named *Usumutsintla* [Land of Apes, *Usumatli* = with reverence, *Usumatsin* = Ape; *tla* = there is, there are, place of]). Arriving, finally, after many difficulties at Tenosique (State of Tabasco), from whence the traveler finds at his disposition small steamers plying to Laguna del Carmen, and thence by sea to Progresso. On this journey I had the luck to discover and photograph several highly interesting and unknown cities, with remarkable monuments and splendid sculptures, some in the neighborhood of Laguna del Peten, others on the right and left shores of the Rio Pasión (*Usumatsintla*).

On my return to Ticul, I found your letters and also one from Mr. Ashmead, which latter I answered, referring him on the subject of aboriginal Syphilis and Lupus to some passages in the ancient Spanish authors.

As to pottery-making, I have observed that it is the work of women solely, who exercise the art, in my opinion, in the ancient manner serving themselves nearly exclusively with the hands and feet and without special instruments. Here at Ticul, it is easy to see them at work, as the industry is a common one in the suburbs.

My collection of ancient earthen vessels is quite interesting, but as you left Ticul in such a hurry I could not show them to you. Several

of my vases have quadrangular inscriptions, of which I have not yet had time to make photographs. Lately the Globus published accounts of several of my smaller expeditions, accompanied by some twenty photographical illustrations which you may perhaps see in the Globus, Nos. 16 and 18, for 1895.

Some days ago, an earthen vessel, full of little implements of worked stone, was found at a hacienda near Ticul. I have been promised the specimens, and will communicate with you in case they turn out to be of interest. From the cave of *Loltun,* I have several very good photographs *Lol* = *Bejuco*, the Haytian name for hanging plants (the name *Vana* is not used in Mexico); *tun* = stone; *Loltun* = stalactites = hanging stones or stones like hanging plants.

I shall be glad to publish, from time to time, in American scientific or popular journals, small articles describing my Yucateckan discoveries, and when my present work of enlarging photographic negatives is finished, shall be ready to prepare for you a series of accounts of my work, accompanied by the necessary celluloid positives from which it is easy to make reversed negatives for the photolithographic process.

Next year I shall return to the States of Tabasco and Chiapas, where I have still to explore several entirely unknown ruins hidden in the wilderness occupied by the *Lacandones* Indians.

—THEOBERT MALER.

Ticul, November 20, 1895.

SCIENTIFIC NEWS.

The Biological Station of the University of Illinois is first to issue its circular for the summer of 1896. The station staff is composed of Professor S. A. Forbes, Director; Dr. C. A. Kofoid, Superintendent; Frank Smith and Adolph Hempell, Zoological Assistants; Dr. A. W. Palmer and C. V. Millar, Chemists; C. A. Hart, Entomologist and B. M. Duggar, Botanist. The station is situated upon the Illinois River near Havana, Ill., and is equipped with every facility for collection and study. There is a floating laboratory sixty feet long and twenty wide, a steam launch, licensed to carry 17 persons, and all the necessary supplies of tables, microscopes, aquaria, nets, chemicals, etc., as well as a specially selected library. As there are accommodations for only 16 in addition to the station staff, applications for the coming summer will be received only from those who have had sufficient experience to place them beyond the need of continuous supervision in their investigations, and, other things being equal, instructors in biology in colleges and high schools will receive the preference. The station will be open

during June, July and August. An incidental fee of $5.00 a month will be charged, and no application for tables should be made for less than two weeks. Board and rooms can be had in Havana at from $4.00 to $5.00 a week. All applications should be addressed to the Director, Professor S. A. Forbes, Urbana, Ill.

The announcement is made that Professor Marshall Ward has been elected to the Chair of Botany in the University of Cambridge, England, to fill the vacancy occasioned by the death (July 22, 1895) of the venerable Professor C. C. Babington.

The University of Cambridge receives the botanical collection of the late Professor Babington.

Mr. F. B. Stead, of Cambridge, England, has been appointed to carry on the investigations of the fisheries at the Plymouth Laboratory, and Mr. T. V. Hodgson as Director's Assistant in the same institution.

After an interregnum of several years, Washburn University, Topeka, Kan., has appointed Dr. G. P. Grimsby, of Columbus, Ohio, to the Chair of Geology and Natural History.

Drs. Walter B. Rankin and C. F. W. McClure, of Princeton, have been advanced to Professorships in Biology in the College of New Jersey.

The Government of the Cape of Good Hope has recently established a geological commission to carry on a survey of that region.

Dr. R. H. True has been appointed Instructor in Pharmacognostical Botany in the University of Wisconsin.

Dr. W. S. Strong, of Colorado, is called to the Chair of Geology in Bates College, Lewiston, Maine.

Bernard H. Woodward has been appointed Curator of the Museum at Perth, W. Australia.

Dr. R. Metzner has been elected Professor of Physiology in the University of Barcelona.

Dr. Dalle-Torre is now Assistant Professor of Zoology in the University of Innsbruck.

Dr. Hans Lenk has been appointed Professor of Geology in Erlangen.

Dr. Ducleaux has been elected President of the Pasteur Institute.

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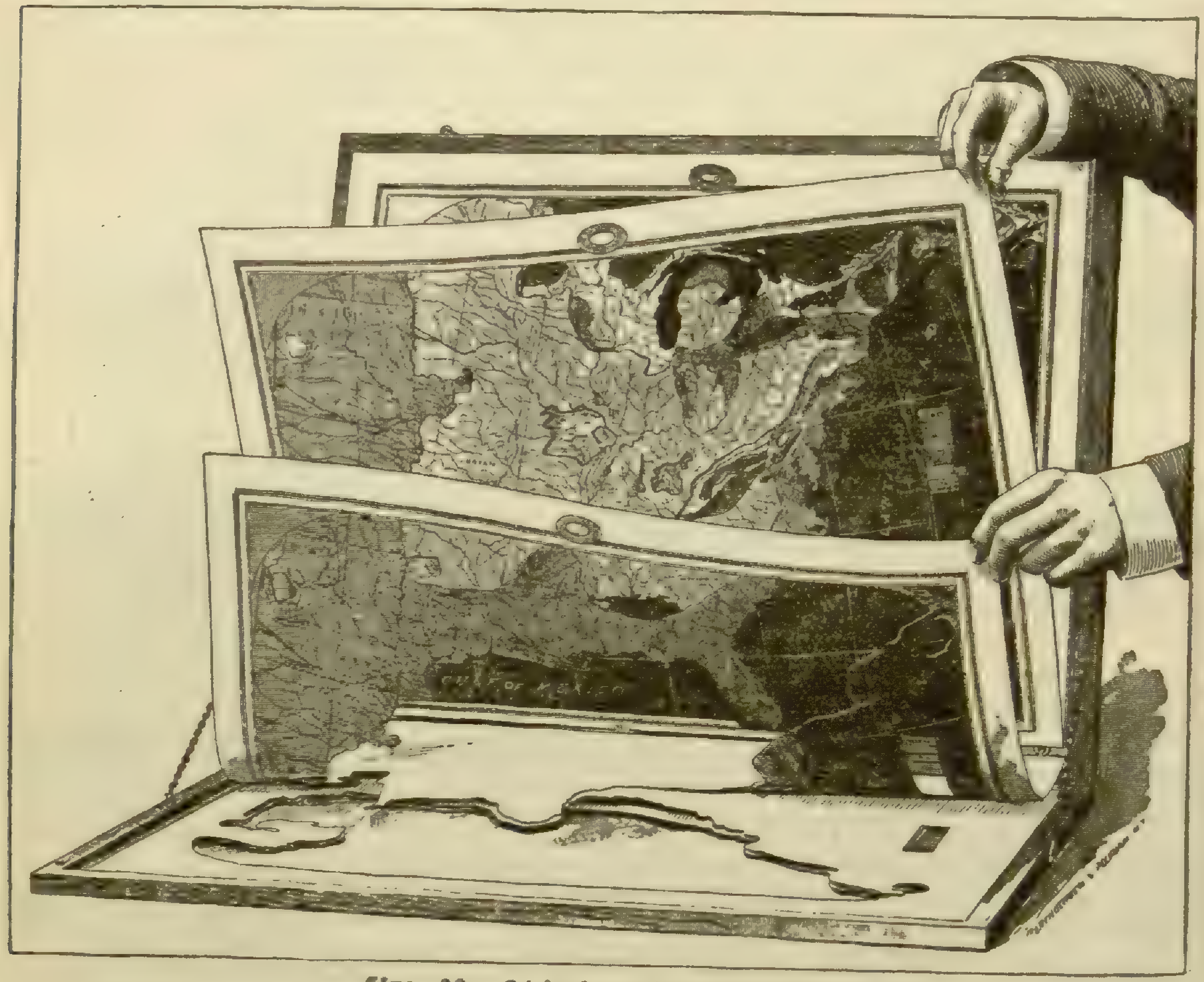

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Prof. T. F. Wright, Explorations in Palestine.
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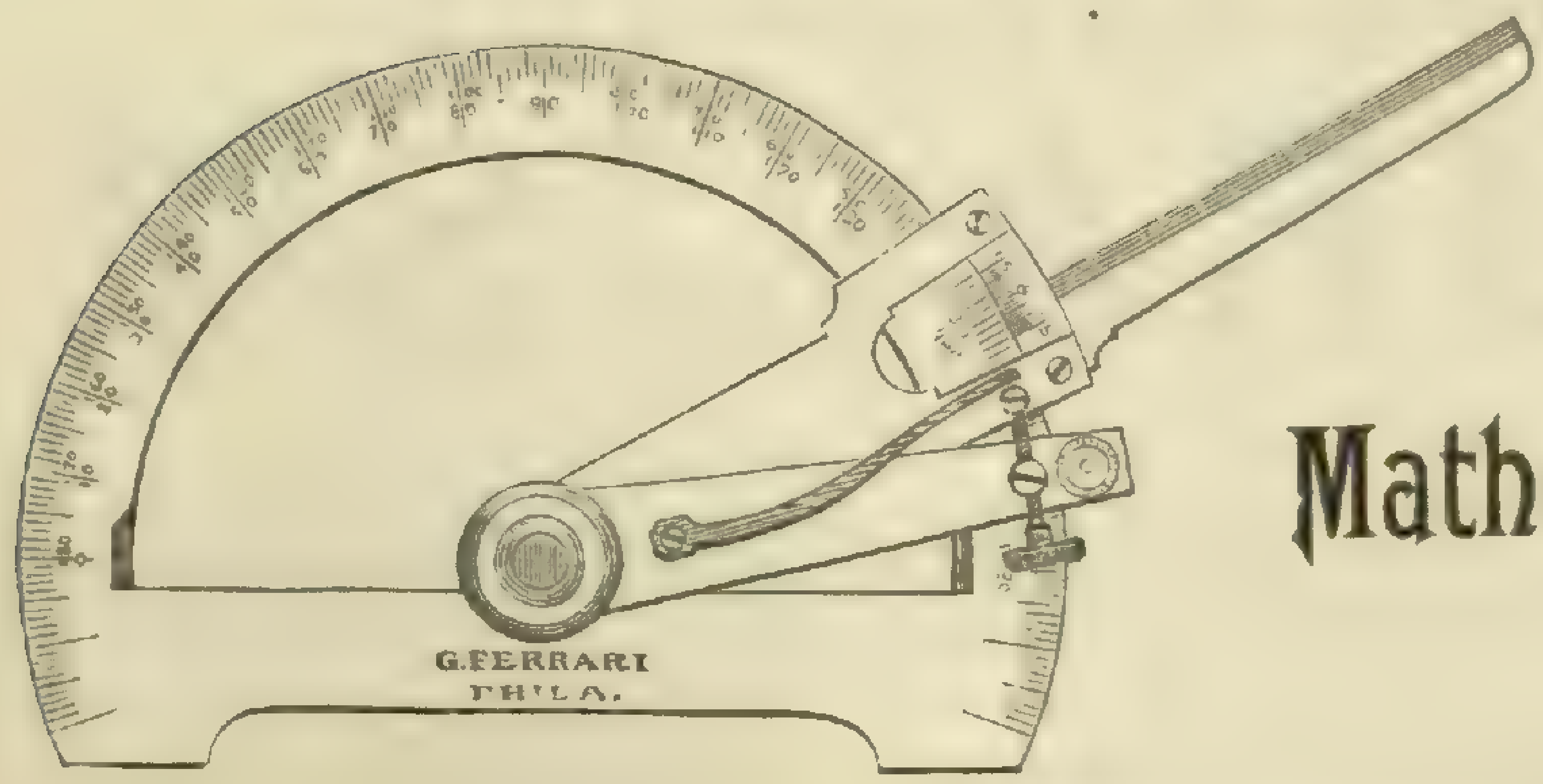

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THE

AMERICAN NATURALIST

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ON HEREDITY AND REJUVENATION.

By Charles Sedgwick Minot.

(*Continued from page* 9.)

III. A Comparison of Larva and Embryo.[8]

It has long been known that animals develop according to two types, appearing in their younger stages, either as larvæ or as embryos. The larvæ lead a free life and must obtain their own food. Embryos, on the contrary, do not lead a free life and are nourished by the yolk accumulated in the parent ovum. There is, of course, no absolute demarcation between the two classes; nevertheless, a general comparison between them establishes several conclusions which throw valuable light upon some recent biological hypothesis.

First of all, it must be remarked that the larval development is primitive, and that the embryonic development has been evolved later. Geologists are able to present two principal supports for this assertion: 1. In the lower animals we encounter only larvæ, never embryos; sponges, colenterates, echinoderms and worms, all pass through the early stages of

[8] Read before the Amer. Soc. of Morphologists, December, 1893.

their ontogeny as larvæ. It would, therefore, be superfluous to linger for the defense of a view which is already accepted by all biologists. 2. The embryonic development depends on the presence of yolk. Now we have learned that the yolk has developed very gradually and in all the lower animals appears only in small quantities. It was not until the increase of yolk material had become enormous, as, for example, in the meroblastic vertebrates, that we find the development completely embryonic in type. With the increase of the yolk comes the gradual transition from larval to embryonic development. Since the embryo is dependent on the yolk, and since the yolk exists only in the higher forms in sufficient quantity, it follows that fully typical embryos can occur exclusively in the higher (later developed) animal types.

The fact that larvæ represent the primitive forms of development, obliges us to conclude that the correctnesss of Weismann's theory of the continuity of germ plasm can be tested better in larvæ than in embryos, since in embryos the relations have undergone profound modifications by secondary changes, which in this connection might easily deceive us.

I do not venture to assert that I know what the present form of Weismann's continuity theory may be; I hold, however, the exact form of this much discussed theory to be nonessential, because, according to my conviction, the theory can in no form be brought into agreement with our present knowledge. Nussbaum founded the theory, and opened the way along which we certainly hope to make great advance. Let me acknowledge the great value and the strictly scientific character of Nussbaum's work; doing this not merely because I esteem it, but also because the unjust attempt has been made to diminish his claim. Nussbaum[9] thought that the germ cells are *direct* decendents of the fertilized ovum, keeping the germinating power. while the rest of the cells developed from the egg are transformed into the tissue of the body. He brought forward several facts which could be interpreted in favor of his theory. By this theory the whole problem of her-

[9] M. Nussbaum, Zur Differenzierung des Geschlechts im Tirreich, Arch. f. Mikrosk. Anatomie, XVIII, 1–121, (1880).

edity and development was stated in an entirely new form. Since this publication of Nussbaum's we are seeking for the explanation of the germinating power, and the propagation of this power; formerly we sought for the causes of the inheritance of parental parts. The difference may be illustrated by the following example. Before Nussbaum we were ruled by Darwin's conception of Pangenesis, and we investigated accordingly for the agency by which the eye of the father reproduced itself in the child. Since Nussbaum we leave Pangenesis behind—it belongs henceforth to the past—and try to determine how the germinal substance behaves, and especially in what way it is perpetuated from the ovum through the following developmental stages, so that it is finally still present for the creation of the next generation. It is the conception of the continuity of the germinal substance which we prize so highly, and owe to Nussbaum.

Larvæ teach us that it cannot be special cells which affect this continuity. In fact, we find the organs of larval life fully differentiated before any sexual organs are recognizable, and indeed, in the majority of known larvæ we cannot recognize even the rudiments of the sexual glands. On the contrary, we find in larvæ unmistakably differentiated locomotive apparatus, such as cilia and often muscle fibres, a digestive canal, sensory organs, and, in many cases, also special excretory organs, and yet, only in a very few and exceptional cases can we distinguish the cells which belong to the future sexual glands. Thus, in regard to the primitive or larval type of development, we cannot say that the germ cells are constantly separated from the somatic cells during the segmentation of the ovum, but must rather draw precisely the opposite conclusion, namely, that the germ cells belong to the tissues which arise latest. We often meet many tissues in larvæ at a time when there is still no indication of germ cells. We find the same relations in embryos also, since in them the principal tissues become recognizable before germ cells are present. This fact was well established for vertebrates many years ago. It is characteristic of Weismann that he long defended the continuity of germ cells, in defiance of the facts. He has since

given up this wrong view and put in its place his hypothesis of the continuity of germ plasm. Of Nussbaum's conceptions, Weismann has left out the fruitful part, and has sown broadcast those ideas which were incapable of fruitful development. He has attempted to defend his notion of the difference between the elements of the embryo destined for the construction of the body, on the one hand, and those elements destined for sexual propagation on the other. Now, since the sexual cells usually develop from somatic cells, he was forced to assume that there is a mysterious substance which he names "*Keimplasma.*" This substance is supposed to store itself in the body by some secret way, to separate itself at command from the histogenic plasm, to appear unchanged and ready to be the exclusive agent of hereditary transmission.

Nussbaum furnished the conception of the continuity of germinal substance, which appears to be of immeasurable importance for the scientific investigation of the phenomena of heredity. But this continuity holds for all cells which arise from the fertilized ovum, as explained in the first section of this article. We must, therefore, seek for the causes of the differentiation of cells, that is to say, for the causes of the production of nerve cells, muscle cells, gland cells, etc, *and* of the production of germ cells.

I will now try to make clear the significance of the comparison between larvæ and embryos for the interpretation of germ cells. This calls for a short digression.

In the course of my investigations on "Senescence and Rejuvenation," of which only the first part has been published (Journal of Physiology, xii, 97), I learned that as cells become older there occurs an increase of the protoplasm in proportion to the nucleus, and I further succeeded in proving, as an essential process in reproduction, the formation of cells with comparatively little protoplasm. Further, it was found probable that a rapid multiplication of cells is only then possible when the cells have small protoplasmatic bodies (Proc. A. A. A. S., XXXIX (1890). We, therefore, have learned that the power of development depends on a special condition of the cell. By these facts I have been led directly to the following hypothesis:

The development of an organism does not depend on a substance stored in special cells, but on a special condition (stage) of organization. As a corollary of this hypothesis may be given this conclusion : *Germ plasm, in Weismann's sense, does not exist.*

According to my view, every part inherits from the germ, and every part of the animal body, as well as its germ cells, possesses the multiplying morphogenetic force, the action of which, however, is inhibited to the condition of the parts themselves. What this condition may be is not yet exactly known, but this much we do know, that the morphogenetic force is found in full activity in cells with little protoplasm. It is indeed highly probable that the slight development of protoplasm in proportion to the nucleus is an unavoidable condition of morphogenesis, or in other words, of the action of heredity. In fact we see that the first processes of development —as I have elsewhere explained (Proc. A. A. A. S., XIX)— show in the most varied cases a remarkable uniformity, for they always accomplish the production of cells with little protoplasm. Compare, in this respect, the vegetation points of plants, the root buds of slips, the budding zones of Annelids, the germinal layers of vertebrates, etc. The condition which allows the morphogenetic or hereditary force to act, arises under differing conditions, of which the fertilization of the ovum is one only.

Weismann tries to make comprehensible to us this one case, that of the fertilized ovum, by a special explanation which is available for no other case. Oscar Hertwig has recently (Zeit und Streitfragen, Heft I) clearly shown that Weismann's explanation is a speculative assumption, which can only be saved from rejection by numerous and often selfcontradictory additional assumptions. As I fully agree with Hertwig's criticism, I need only refer to his essay.

We will return to our proper theme. The next point is to determine whether there is a difference in the condition of the cells, as, regards their capacity for development, between larvæ, on the one hand, and embryos on the other. It can be proved that this is the case, by the following considerations. So far as we yet know, it is chiefly two factors which inhibit

development: *first*, the increase of protoplasm; *second*, the progress of organization, i. e., of differentiation.

As I was about to close this article, I received through the kindness of the author, Nussbaum's address on differation, in which he has defended essentially the same views as those which I hold. Such an agreement is of great value to me.

Now we know that larvæ are animal forms which have to obtain their own food and to protect themselves against enemies, and therefore are provided with differentiated tissues. Embryos, on the contrary, take their nutriment simply from the ovum, and the cells continue for a long time, developing and multiplying, while the protoplasm of the single cells increases very slightly, and *the beginning of the differentiation proper is correspondingly postponed.* I believe that we here have to deal with causal relations. From the actual relations just described, I conclude that the most essential difference hitherto known between larvæ and embryos, is to be found in the differing lengths of the period of multiplication of undifferentiated cells. In consequence of the shorter duration of the period in larvæ they have a much smaller total number of undifferentiated cells than embryos, or reversely expressed, embryos are much better equipped with material for the construction of the adult body, than are larvæ. As already stated, embryos are produced by the higher animals. This fact finds its explanation in the relations just described, because the increased number of undifferentiated, or so called embryonic cells, is precisely the necessary preliminary condition of the greater complexity of the differentiation by which the animal becomes more highly organized.

For the sake of clearness I have put aside all complications which might come in to play. It goes without saying, that the relations, in many respects, are by no means simple, nevertheless, the main conclusion above given seems a secure gain.

I therefore interpret the embryo as a device to render possible the increase of undifferentiated cells, and consequently a higher ultimate organization. The origin of this device is conditioned by a supply of food independent of the embryo.

From our present standpoint it is a matter of indifference whether the independent food supply comes from the yolk or from the uterus, however important the difference may be from other points of view.

It is to be further noted that our interpretation of the significance of the embryo is also opposed to Weismann's theory of germ plasm, because it emphasizes the importance of the *condition* as opposed to the assumption of a germinal substance or plasm. This road also leads to the conclusion reached above by other ways, the conclusion, namely: Reproduction involves rejuvenation, and rejuvenation is characterized by the production of cells with little, and that little not differentiated, protoplasm. Since rejuvenated cells arise by asexual as well as by sexual reproduction, since they appear in much greater numbers in embryos than in larvæ, and since they may be interpolated, as in the pupæ of butterflies, in the midst of the development of an individual, we must admit that the hereditary impulse (*vererbende Kraft*) is distributed in very different cells and is probably distributed equally through all cells. Hertwig has reached the same conclusion, with which Weismann's theory of germ plasm cannot be made to agree.

As Weismann has neglected the problem of rejuvenation, he has necessarily often gone astray in his discussion of phenomena in which rejuvenation plays the principal role. One is astonished at the slight attention bestowed on rejuvenation, when one recalls that it is the central problem of all questions of heredity treated by him.

Rejuvenation is one of the principal phenomena of life, and the rejuvenated condition of the cell is probably an unavoidable preliminary of heredity. We know that at least one *anatomical* sign of the rejuvenated condition is to be found in the preponderance of the nucleus in proportion to the protoplasm; a second *anatomical* sign is found in the structure of the protoplasm, which, in young cells always remains without differerentiation. The chief *physiological* sign of rejuvenation in cells which we as yet know is the power of rapid multiplication. Thus, we see, in case of sexual rejuvenation, that the

development of the fertilized ovum begins with an excessive proliferation of the nuclei, by which numerous cells are created, each with little protoplasm. Histogenetic differentiation begins later. The asexual rejuvenation has a similar course, but needs more thorough investigation.

Now differentiation is the sign of inheritance, and this morphological inheritance cannot develop itself fully until the senescence of the cells becomes recognizable by the growth of their protoplasm. On the other hand, we see complete inheritance develop itself, after preceeding rejuvenation. Accordingly we gain two conceptions: *first,* the hereditary impulse belongs to the inherent and constant properties of cells in general; *second,* the activity of their impulse may be inhibited by the condition of the cells. My view may be expressed in the following way: Somatic cells are simply cells in which the activity of the hereditary impulse is inhibited in consequence of their senescence, or, in other words, differentiation; but under suitable conditions the somatic cells may pass over into the rejuvenated stage, and thereupon develop the most complete hereditary possibilities.

The importance of rejuvenation must also be recognized when we consider the phylogenetic origin of single organs. Let us take a simple example. We may safely assume that the ancestors of mammals possessed a smooth skin, and that the covering of hairs is a new acquisition. Each hair is the product of a local growth. If we investigate the germ of a hair, we find that it consists of rejuvenated cells, that is to say, of cells with little protoplasm, or, as we are accustomed to say, of the embryonic type. Thus the formation of hairs depends on numerous centers of rejuvenation. In the multiplication of striped muscle fibres we find the agents to be the muscle buds, which are small, protoplasmatic structures, with relatively numerous nuclei. If we observe a developing gland, let us say a pancreas or a sweat gland, we find the rudiment to consist of rejuvenated cells; the cells multiply rapidly, and after the organ has its essential form, the histogenetic differentiation begins. It would be easy to multiply such examples a thousandfold.

The consideration of the role of rejuvenation in the origin of organs leads us to the theory of POST-SELECTION (Nachauslese). The theory is by no means new, but I wish to emphasize its far reaching importance. The preceding discussion teaches us to divide the origin of a new morphological part into two stages. The first stage is the development of the rudiment (*anlage*) by multiplication of the cells. The second stage is characterized by the gradual differentiation of the cells, by which they become capable of their ultimate functions. Especially in embryos is the difference in time very marked between the formation and the differentiation of the "*Anlage.*" Now it is evident that the undifferentiated "*Anlage*" is not useful, but becomes useful later. The formation and conservation of the "*Anlage,*" therefore, are due to selection, working, not directly upon the "*Anlage,*" but indirectly through preservation of the fully developed organ. The conception advanced is very simple and appears to me a necessary consequence of our knowledge. For the conception itself there has been hitherto to no definite term, I propose, therefore, to call it "*Post-selection*" (in German, "*Nachauslese*). To avoid possible misunderstanding, I give another example of post-selection. A parasitic wasp lays its egg in a certain caterpillar; the mother wasp gains no advantage, natural selection does not touch her, but only her progeny, the wasp larva. Nevertheless, the survival of the fittest rules.

In conclusion, I should like to direct the reader's attention to a problem which, so far as I am acquainted with the literature of biology, has been left almost unconsidered. This present translation enables me to insert a qualification of the preceding sentence, which ought to have been inserted in the original article, namely, that the problem has been the subject of important discussions by Hyatt, Cope and a very few others among paleontologists. I am glad to be able to refer to the article by Professor Hyatt, (see Jan. Naturalist) and presents the paleontological theory of the loss of ancestral characteristics. The problem above referred to is the *problem of lost characteristics,* which seems to me one of the fundamental problems of the doctrine of evolution, because we cannot un-

derstand the development of the higher organisms until this problem is solved. Everybody is writing about the origin of new organs, and we take lively pleasure in discussions about acquired characteristics. But if we consider the circumstances closely, we recognize that the loss of ancestral characteristics almost equals in importance the acquisition of new characteristics for the formation of new species. We assume that man had fishlike ancestors, and we strengthen ourselves in this belief by the comparison so often made between the human embryo, on the one side, and the adult fish on the other. But if the comparison be impartial we are forced to admit that nearly everything which is most characteristic of the fish is conspicuously lacking in the human embryo. Taking the embryo at the stage when the gill clefts have their maximum development, we find the following relations: the body is not straight but coiled up, and this coiling up is indispensable, in order to bring about the proper distribution of the human nerves, blood vessels, and so forth; the gill clefts are closed; gills are wanting; the digestive canal has no glands; the epidermis has no scales; the chorda dorsalis does not form a large axis of the body, but is a minute string of cells. In short, the *Biogenetisches Grundgesetz* (Recapitulation theory or von Baer's law, according to Adam Sedgwick) is scarcely half true. I have previously defended this conclusion at a meeting of the American Society of Morphologists, in December, 1893, Subsequently, but independently, Adam Sedgwick has reached a similar conclusion, see his paper "*On the Law of Development, etc.*" (Quart. Jour. Micros. Sci., XXXVI, 35). Were it not, as above implied, that the departures from the fish type are in great excess, there would be no embryo at all, and consequently no man, for the adult form is a consequence of the embryonic. The embryo is the mechanical cause of the adult body. How has the disappearance of the ancestral fish characteristics been effected? The question remains unanswered. It will, perhaps, be replied "through disuse" or "through panmixia." But "disuse" is merely a name, not an explanation of the phenomena. Panmixia is an hypothesis erected on nothing. In fact, this hypothesis as-

sumes that the majority of variations fall below the value maintained by natural selection, and consequently that when the influence of natural selection is eliminated (as in disuse), the mere variation will bring the traits concerned to disappearance. It marks Weismann's style of thought to find that he has entirely omitted to determine whether his assumption was correct, and nevertheless in his book, "The Germ Plasm," presents panmixia as an established law. As a matter of fact, the statistics of variations which we already have, show that his assumption is erroneous, and that it is equally probable that mere variation will magnify a characteristic as it is that it will diminish it.

Let us return to the embryo. The following hypothesis may be advanced:

The loss of ancestral characteristics in the embryo is due to post-selection, the cells being kept in a rejuvenated stage, in order that they may afterwards accomplish new differentiations.

This conclusion follows directly from the preceding considerations, and, therefore, needs no further defense.

IV. Concluding Remarks.

The views presented in the preceeding sections are intimately connected one with another and collectively determine our conception of the process of heredity. The conception concerns only the process and not the essential character or cause of heredity. According to my view, heredity exists in all cells, but its display is inhibited by organization of the living substance, and can be complete only in embryonic cells; embryonic cells arise under very various conditions. That which is novel in this theory is the significance attributed to embryonic cells. Embryonic cells I prefer to designate as rejuvenated cells.

The theory above presented is an unavoidable consequence of the facts known, and stands in absolute contradiction with Weismann's theory of the germ plasm.

I have read with the greatest conscientiousness every article hitherto published by Weismann, which deals with his theories of heredity. My final impression from this study is that

the "*Theory of Germ Plasm*" corresponds to the personal inclinations of its author and is in no sense a logical deduction won by the collation of facts. The assumption of a difference between germ plasm and histogenic plasm explains nothing. Even according to Weismann's own exposition it explains nothing, for the supposed phenomena which the assumption is said to explain, according to Weismann, do not exist. According to him, the circumstances are the following: The phenomena due to the germ plasm do not occur in somatic cells, therefore they have a different plasm, namely, histogenic; further, these phenomena do occur in somatic cells, therefore, they have germ plasm. Attention must be directed also, and explicitly, to the fact that Weismann offers no *observations* to support his fundamental assumption. His theory is mystical to an extreme degree. In Weismann's book, "*The Germ plasm,*" one finds one hypothesis after another in order to support his tottering first hypothesis—germ plasm and histogenic plasm are special and separate substances. I demand of Weismann that he lay aside *all his hypotheses,* and present to us solely the *facts,* which support his theory of germ plasm. Then he will learn, as other investigators have already learned, that his hypothesis has been built up without sufficient foundation.

Let an investigator enquire for a possibility of testing the existence of the "Ids," "Biophors," "Determinants," etc., asserted by Weismann, and he will discover that the whole fabric is woven by speculative imagination. Confirmation of his ideas has, strictly speaking, not been attempted by Weismann. Indeed, confirmation is altogether impossible, for his conceptions are far beyond the limits of present human means of investigation.

It is time to finally discard a theory which leads astray and which, although it arose without scientific justification, is again and again pushed to the front by its promulgator. It is a scientific duty to take an unhesitating stand against Weismann's theory, for only so can it become known that those who have specially occupied themselves with the problem of heredity reject Weismann's theory of germ plasm unconditionally.

APPENDIX.

The Theory of Panplasm.

It appears desirable that the modern theory of heredity should be designated by a brief and appropriate name, and accordingly I propose the term "*Panplasm*," and that the theory be called "The Theory of Panplasm." By panplasm will be understood the physical basis of hereditary transmission, which is supposed to be distributed through all cells, and which accounts for the phenomena of sexual reproduction, regeneration and asexual reproduction. Panplasm is not a collection of gemmules or biophors. The term "panplasm" was first used by me at a meeting of the Society of Arts, in Boston, November 14, 1895.

On another occasion I hope to discuss the theories of pangenesis and panplasm in their historical aspects.

THE FORMULATION OF THE NATURAL SCIENCES.[1]

By E. D. Cope.

Formulation is the method of presentation of the forms of our thoughts. Our observations of the facts of material nature are embodied in such classifications as we think best express their relations, and by means of these classifications expressed in language, we convey to others our conclusions in the premises. As the vehicle of presentation, formulation is one of the aspects of language, which as the medium of communication between men, enables them to accumulate knowledge. It is highly important then that the system of formulation should be uniform, so as to convey definite meaning and preserve the truth. The vast number of facts to be marshaled in orderly array, which constitute the natural sciences, require a

[1] Presidential address delivered before the American Society of Naturalists in Philadelphia, Dec. 26th, 1895.

correspondingly complex and exact formulation. The advent of the doctrine of evolution into the organic sciences involves the necessity of making such readjustments of our method of formulation as may be called for. It is with reference to this condition and the present action of naturalists regarding it, that I address you to-day. The subject may be considered under the three heads of Taxonomy, Phylogeny, and Nomenclature.

I. Taxonomy.

Taxonomy or classification is an orderly record of the structural characters of organic beings. The order observed is an order of values of these characters. Thus we have what we call specific or species value, generic value, family value, and so on. These values are not imaginary or artificial, as some would have us believe, but they are found in nature. Their recognition by the naturalist is a matter of experience, and the expression of them is a question of tact. Their recognition rests on a knowledge of morphology, or the knowledge of true identities and differences of the parts of which organic beings are composed. The formulation of these values in classification foreshadows the evolutionary explanation of their origin, and is always the first step necessary to the discovery of a phylogeny.

Taxonomy, then, is, and always has been, an arranging of organic beings in the order of their evolution. This accounts for the independence of the values of taxonomic characters, of any other test. Thus, no character can be alleged to be of high value because it has a physiological value, or because it has no physiological value. A physiological character may or may not have a taxonomic value. The practiced taxonomist finds a different test of values, which is this. He first endeavors to discover the series of organic forms which he studies. He learns the difference between its beginning and its ending. His natural divisions are the steps or stages which separate the one extremity from the other. The series may be greater or they may be lesser, i. e., more or less comprehensive, and it is to the series of different grades that we give the different names of the genus, family, order, etc.

We know that the characters of specific value in given cases are usually more numerous than those of higher groups. We know that they are matters of proportions, dimensions, textures, patterns, colors, etc., which are many. The characters of the higher groups, on the contrary, are what we call structural, i. e., the presence, absence, separation or fusion of elemental parts, as estimated by a common morphologic standard; and it is the business of the morphologist to determine each case on this basis. In these characters lies the key to the larger evolution, that of the higher aggregations of living things. On the contrary, the study of the origin of species characters gives us the evolution of species within the genus, but of nothing more, except by inference.

Classification, then, is a record of characters, arranged according to their values. There still lingers, in some quarters, a different opinion. This holds that there is such a thing as a "natural system," as contrasted with "an anatomical system." Examination shows that the supporters of this view suppose that there is some bond of affinity between certain living beings which is not expressed in anatomical characters. A general resemblance apparent to the eye is valued by them more highly than a structural character. If this "general appearance" is analyzed, however, it is found to be simply an aggregate of characters usually of the species type, which by no means precludes the presence of anatomical differences. And these anatomical differences may indicate little relationship, in spite of the general resemblance of the species concerned, or they may have only the smallest value attached to such characters, i. e., the generic. It is with regard to the generic characters that the chief difference of practice exists. But it is clear that the record of this grade of characters cannot be modified by questions of specific characters. The two questions are distinct. Both represent nature, and must be formulated. In fact, I have long since pointed out that the same species, so far as species characters go, may have different generic characters in different regions. Also that allied species of different genera may have more specific characters in common than remote species of the same genus.

The anticipation naturally intrudes itself that the characters which distinguish the steps in a single evolutionary or genealogical line must disappear with discovery, and new ones appear, and that they must be all variable at certain geological periods, and hence must become valueless as taxonomic criteria. And it is therefore concluded that our systematic edifice must lose precision and becomes a shadow rather than a reality. I think that as a matter of fact this will not be the result, and for the following reasons. In the first place, when, say all the generic forms of a genealogical line, shall have been discovered, we will find that each one of them will differ from its neighbor in one character only. This naturally follows from the fact that two characters rarely, if ever, appear and disappear contemporaneously. Hence, generic characters will not be drawn up so as to include several points. For a while, there will be found to be combinations of two or three characters which will serve as definitions, but discovery will relegate them to a genus each. Each of these characters will be found to have what I have called the "expression point," or the moment of completeness, before which it cannot be said to exist. In illustration I cite the case of the eruption of a tooth. Before it passes the line of the alveolus it is not in use; it is not in place as an adult organism. When it passes that line it has become mature, has reached its expression point, comes into functional use, and may be counted as a character. Such will be found to be the case with all separate parts; there always will be a time when they are not completed, and then there will be a time when they are. These lines, then, will always remain as our boundaries, as they are now, for all natural divisions from the generic upwards. This condition cannot exist in characters of proportionate dimensions, which which will necessarily exhibit complete transitions in evolution. Hence, proportions alone can only be used ultimately as specific characters.

Some systematists desire to regard phyletic series as the only natural divisions. This may be the ultimate outcome of paleontologic discovery, but at present such a practice seems to me to be premature. In the first place, as all natural divisions

rest on characters, we must continue to depend on their indications, no matter whether the result gives us phyletic series or not. In the next place, we must remember that we have in every country interruptions in the sequence of the geological formations, which will give us structural breaks until they are filled. There are also periods when organic remains were not preserved ; these also will give us interruptions in our series. So we shall have to adhere to our customary method without regard to theory, and if the phyletic idea is correct, as I believe it to be, it will appear in the final result, and at some future time.

Authors are frequently careless in their definitions. Very often they include, in the definition of the order, characters which belong in that of the family, and in that of the family those that belong in the genus. Characters of different values are thus mixed. The tendency, especially with naturalists who have only studied limited groups, is to overestimate the importance of characters. Thus the tendency is to propose too many genera and other divisions of the higher grades. In some groups structure has been lost sight of altogether, and color patterns, dimensions, and even geographical range, treated as characters of genera. As the mass of knowledge increases, however, the necessity for precision will become so pressing that this kind of formulation will be discarded, and definitions which mean something will be employed. Search will be made especially for that one character which the nature of the series renders it probable will survive, as discoveries of intermediate forms are successively made, and here the tact and precision of the taxonomist has the opportunity for exercise. In the selection of these characters, one problem will occasionally present itself. The sexes of the same species sometimes display great disparity of developmental status, sometimes the male, but more frequently the female, remaining in a relatively immature stage, or in others presenting an extraordinary degeneracy. In these cases the sex that displays what one might call the genius, or in other words, the tendency, of the entire group, will furnish the definitions. This will generally be that one which displays the most numerous char-

acters. In both the cases mentioned the male will furnish these rather than the female; but in a few cases the female furnishes them. The fact that both sexes do not present them does not invalidate them, any more than the possession of distinct reproductive systems would refer the sexes to different natural divisions.

I have seen characters objected to as of little value because they were absent or inconstant in the young. I only mention the objection to show how superficially the subject of taxonomy may be treated. So that a character is constant in the adult, the time of its appearance in development is immaterial in a taxonomic sense, though it may have important phylogenetic significance.

II. Phylogeny.

The formulation of a phylogeny or genealogy involves, as a preliminary, a clear taxonomy. I refer to hypothetical phylogenies, such as those which we can at present construct are in large part. A perfect phylogeny would be a clear taxonomy in itself, so far as it should go, did we possess one; and such we may hope to have ere long, as a result of paleontological research. But so long as we can only supply parts of our phyletic trees from actual knowledge, we must depend on a clear analysis of structure as set forth in a satisfactory taxonomy, such as I have defined above.

Confusion in taxonomy necessarily introduces confusion into phylogeny. Confusion of ideas is even more apparent in the work of phylogenists than in that of the taxonomists, because a new but allied element enters into the formulation. It is in the highest degree important for the phylogenist, whether he be constructing a genealogic tree himself or endeavoring to read that constructed by some one else, to be clear as to just what it is of which he is tracing the descent. Is he tracing the descent of species from each other, or of genera from each other, or of orders from each other, or what? When I trace the phylogeny of the horse, unless I specify, it cannot be known whether I am tracing that of the species *Equus caballus*, or that of the genus Equus, or that of

the family Equidæ. When one is tracing the phylogeny of species, he is tracing the descent of the numerous characters which define a species. This is a complex problem, and but little progress has been made in it from the paleontologic point of view. Something has been done with regard to the descent of some living species from each other. But when we are considering the descent of a genus, we restrict ourselves to a much more simple problem, i. e., the descent of the few simple characters that distinguish the genus from other genera. Hence, we have made much more progress in this kind of phylogeny than with that of species, especially from the paleontologic point of view. The problem is simplified as we rise to still higher divisions, i. e., to the investigation of the origin of the characters which define them. We can positively affirm many things now as to the origin of particular families and orders, especially among the Mammalia, where the field has been better explored than elsewhere.

It is in this field that the unaccustomed hand is often seen. Supposing some phyletic tree alleges that such and such has been the line of descent of such and such orders or families, as the case may be; soon a critic appears who says that this or that point is clearly incorrect, and gives his reasons. These reasons are that there is some want of correspondence of generic characters between the genera of the say two families alleged to be phyletically related. And this want of correspondence is supposed to invalidate the allegation of phyletic relation between the families. But here is a case of irrelevancy; a generic character cannot be introduced in a comparison of family characters. In the case selected, the condition is to be explained by the fact that although the families are phyletically related, one or both of the two juxtaposed genera through which the transition was accomplished has or have not been discovered. The same objection may be made against an allegation of descent of some genus from another, because the phyletic relation between the known species of the two genera cannot be demonstrated. I cite as an example the two genera, Hippotherium and Equus, of which the latter has

been asserted with good reason to have descended from the former. It has been shown, however, that the *Equus caballus* could not have descended from the European *Hippotherium mediterraneum*, and hence some writers have jumped to the conclusion that the alleged phyletic relation of the two genera does not exist. The reasons for denying this descent are, however, presented by specific characters only, and the generic characters are in no way affected. Further, we know several species of Hippotherium which could have given origin to the *Equus caballus* probably through intermediate species of Equus.

Some naturalists are very uncritical in criticising phylogenies in the manner I have just described. They often neglect to ascertain the definitions given by an author to a group alleged by him to be ancestral; but fitting to it some definition of their own, proceed to state that the ancestral position assigned to it cannot be correct, and to propose some new division to take its place. It is necessary to examine, in such cases, whether the new group so proposed is not really included in the definition of the old one which is discarded.

The fact that existing genera, families, etc., are contemporary need not invalidate their phyletic relation. Group No. 1 must have been contemporary with group No. 2, at the time that it gave origin to the latter, and frequently, though always, a certain number of representatives of group No. 1 have not changed, but have persisted to later periods. Some genera, as, e. g., Crocodilus, have given origin to other genera (i. e., Diplocynodon) and have outlasted it, for the latter genus is now extinct. The lung fishes, Ceratodus, are probably ancestral to the Lepidosirens, but both exist to-day. Series of genera, clearly phyletic, of Batrachia Salientia, are contemporaries. Of course we expect that the paleontologic record will show that their appearance in time has been successive. But many ancestors are living at the same modern period as their descendents, though not always in the same geographic region.

III. Nomenclature.

Nomenclature is like pens, ink and paper; it is not science, but it is essential to the pursuit of science. It is, of course, for convenience that we use it but it does not follow from that that every kind of use of it is convenient. It is a rather common form of apology for misuse of it to state that as it is a matter of convenience, it makes no difference how many or how few names we recognize or use. An illustration of this bad method is the practice of subdividing a genus of many species into many genera, simply because it has many species. The author who does this ignores the fact that a genus has a definite value, no matter whether it has one or five hundred species. I do not mean to maintain that the genus or any other value has an absolute fixity in all cases. They undoubtedly grade into each other at particular places in the system, but these cases must be judged on their own merits. In general there is no such gradation.

Nomenclature is then orderly because the things named have definite relations which it is the business of taxonomy, and nomenclature its spokesman, to state. Here we have a fixed basis of procedure. In order to reach entire fixity, a rule which decides between rival names for the same thing is in force. This is the natural and rational law of priority. With the exception of some conservative botanists, all naturalists are, so for as I am aware, in the habit of observing this rule. The result of a failure to do so is self evident. There is, however, some difference of opinion as to what constitutes priority. Some of the aspects of the problem are simple, others more difficult. Thus there is little or no difference of opinion as to the rule that the name of a species is the first binomial which it received. This is not a single date for all species, since some early authors who used trinomials and polynomials occasionally used binomials. A second rule which is found in all the codes, is that a name in order to be a candidate for adoption, must be accompanied by a descriptive diagnosis or a plate. As divisions above species cannot be defined by a plate, a description is essential in every such case.

It is on the question of description that a certain amount of difference of opinion exists. From the codes of the Associations for the Advancement of Science, and of the Zoological Congresses, no difference of opinion can be inferred, but the practice of a number of naturalists both zoologists and paleontologists in America, and paleontologists in Europe, is not in accord with the rule requiring definition of all groups above species. It has always appeared to me remarkable that a rule of such self evident necessity should not meet with universal adoption. However, the objections to it, such as they are, I will briefly consider. It is alleged that the definitions when first given are more or less imperfect, and have to be subsequently amended, hence it is argued they have no authority. However, the first definitions, if drawn up with reference to the principles enumerated in the first part of this address, need not be imperfect. Also an old time diagnosis of a division which we have subsequently found it necessary to divide, is not imperfect on that account alone, but it may be and often is, the definition of a higher group. But you are familiar with all this class of objections, and the answers to them, so I will refer only to the positive reasons which have induced the majority of naturalists to adhere to the rule.

It is self evident that so soon as we abandon definitions for words, we have left science and have gone into a kind of literature. In pursuing such a course we load ourselves with rubbish, and place ourselves in a position to have more of it placed upon us. The load of necessary names is quite sufficient, and we must have a reason for every one of them, in order to feel that it is necessary to carry it. Next, it is essential that every line of scientific writing should be intelligible. A man should be required to give a sufficient reason for everything that he does in science. Thus much on behalf of clearness and precision. There is another aspect of the case which is ethical. I am aware that some students do not think that ethical considerations should enter into scientific work. To this I answer that I do not know of any field of human labor into which ethical considerations do not necessarily enter. The reasons for sustaining the law of priority are partly

ethical, for we instinctively wish to see every man credited with his own work, and not some other man. The law of priority in nomenclature goes no further in this direction than the nature of each case requires. Nomenclature may be an index of much meritorious work, or it may represent comparatively little work; but it is to the interest of all of us that it be not used to sustain a false pretence of work that has not been done at all. By insisting on this essential test of honest intentions we retain the taxonomic and phylogenetic work within the circle of a class of men who are competent to it, and cease to hold out rewards to picture makers and cataloguers.

Another contention of some of the nomenclators who use systematic names proposed without description, is, that the spelling in which they were first printed must not be corrected if they contain orthographical and typographical errors. That this view should be sustained by men who have not had the advantage of a classical education, might not be surprising, although one would think they would prefer to avoid publicly displaying the fact, and would be willing to travel some distance in order to find some person who could help them in the matter of spelling. But when well educated men support such a doctrine, one feels that they have created out of the law of priority a fetish which they worship with a devotion quite too narrow. The form of our nomenclature being Latin, the rules of Latin orthography and grammar are as incumbent on us to observe, as are the corresponding rules of English grammar in our ordinary speech. This cult so far as I know, exists only in the United States and among certain members of the American Ornithologists Union. The preservation of names which their authors never defined; of names which their proposers misspelled; of names from the Greek in Greek instead of Latin form; of English hyphens in Latin composition; and of hybrid combinations of Greek and Latin, are objects hardly worth contending for. Some few authors are quite independent of rules in the use of gender terminations, but I notice the A. O. U. requires these to be printed correctly. Apart from this I notice in the second edition of their check list of North American Birds just issued, only

eighteen misspellings out of a total number of 768 specific and subspecific names, and the generic and other names accompanying. These are of course not due to ignorance on the part of the members of this body, some of whom are distinguished for scholarship, but because of an extreme view of the law of priority.

In closing I wish to utter a plea for euphony and brevity in the construction of names. In some quarters the making of such names is an unknown art. The simple and appropriate names of Linneus and Cuvier can be still duplicated if students would look into the matter. A great number of such names can be devised by the use of significant Greek prefixes attached to substantiatives which may or may not have been often used. Personal names in Greek have much significance, and they are generally short and euphonious. The unappropriated wealth in this direction is so great that there is really no necessity for poverty in this direction. It should be rarely necessary, for instance, to construct generic names by adding prefixes and suffixes of no meaning to a standard generic name already in use.

SOME LOCALITIES FOR LARAMIE MAMMALS AND HORNED DINOSAURS.

By J. B. Hatcher.

It is the purpose of this paper to give brief but accurate descriptions of the localities for the most important and best preserved specimens of Laramie mammals and horned and other dinosaurs collected by the writer for the U. S. Geological Survey, and now carefully stored in the Yale Museum at New Haven; with a map of the most important locality at present known and suggestions to collectors visiting this, or other localities as to the most promising places and best methods to be employed in order to attain the greatest degree of success.

History of the Discovery of Laramie Horned Dinosaurs.

As early as 1872, Professor Cope[1] described under the name of *Agathaumas sylvestre* a portion of the skeleton of a horned dinosaur from Laramie beds near Black Butte in southwestern Wyoming. In various publications from 1874–1877 which appeared in THE AMERICAN NATURALIST, Proceedings Philadelphia Academy Sciences and Bulletins of the U. S. Geological Survey, Cope has added much to our knowledge of these strange forms, chiefly from material collected by himself and Mr. Charles H. Sternberg from the vicinity of Cow Island on the upper Missouri River in Montana.

In 1887 a new locality for horned dinosaurs was found near Denver, Colorado, by Mr. George L. Cannon. The most important specimen, consisting of a pair of horn cores, was sent to Professor Marsh for identification and description. They were not characteristic, and owing to their striking resemblance to the horns of certain fossil Bisons, they were referred by Marsh to that genus and described under the name of *Bison alticornis*; the beds in which they were found being referred to late Pliocene and denominated the *Bison beds.*[2]

In 1888 the writer secured in the same locality in which Cope had operated in 1875 and 1876 on the upper Missouri, parts of several skulls of a horned dinosaur, some of which Marsh has described, creating for them a new genus *Ceratops*, and several new species. A comparison of the types of Cope's *Monoclonius recurvicornis* and Marsh's *Ceratops montanus*, both from the same locality in Montana, would doubtless establish the generic identity of the two.

Not until 1889 was a locality found where remains of these animals were sufficiently abundant and well preserved to afford material which would give us an adequate idea of their structure and habits. In the fall of 1888 the writer's attention was called to a pair of horncores belonging to Mr. C. A. Guernsey, of Douglas, Wyoming. Upon inquiry it was learned that they had been taken from a huge skull found by Mr. E. B. Wilson

[1] Proc. Am. Phil. Soc., 1872, p. 482.

[2] See notice of new Fossil Mammals, Am. Jour. Sci., Oct., 1887.

on Buck Creek, some of 35 miles north of Lusk, Wyoming. Early in the spring of 1889 the writer proceeded to Lusk, near which place Mr. Wilson still lived, and easily succeeded in getting that most accomodating gentleman to show him the skull from which he had taken the horns. This has proved a most important locality, and material obtained from it has increased many fold our knowledge of the Laramie reptilian and mammalian faunas. In the nearly four years spent by the writer in working these beds, 31 skulls and several fairly complete skeletons of horned dinosaurs were secured, besides two quite complete skeletons of *Diclonius* (*Claosaurus*), about 5000 isolated jaws and teeth of Laramie mammals and numerous remains of other dinosaurs, turtles, lizards, birds and fishes, as well as extensive collections of freshwater invertebrates from the same beds. In all over 300 large boxes of fossils were collected for the U. S. Geological Survey, and are now carefully stored in the Yale Museum, many of them as yet unopened.

At present remains of horned dinosaurs are known from only four widely separated localities; one of these, that of Black Butte, Wyoming, is west of the main range of the Rocky Mountains, and the other three including the Denver locality in Colorado; the Converse Co. locality in the extreme eastern portion of central Wyoming, and the Judith River or Cow Island locality in northern Montana, lie east of the main range. There are other localities known to the writer, but they are as yet of minor importance, since little collecting has been done in them and no material has been described from them. They will be referred to later.

The Ceratops Beds.

In the American Journal of Science for December, 1889, Professor Marsh applied the name *Ceratops beds* to certain strata in the west from which horned dinosaurs had been secured. He did not then, nor has he at any time since, designated just what he considered the geographical distribution of these beds nor their upper and lower delimitations in the geological scale. In order that the reader may not be misled in regard to Professor Marsh's position on this question I will quote him some-

what fully. In speaking of the horned dinosaurs in the publication just cited he says: "The geological deposits, also, in which their remains are found have been carefully explored during the past season, and the known localities of importance examined by the writer, to ascertain what other fossils occur in them, and what were the special conditions which preserved so many relics of this unique fauna.

"The geological horizon of these strange reptiles is a distinct one in the upper Cretaceous, *and has now been traced nearly eight hundred miles along the eastern flank of the Rocky Mountains.* It is marked almost everywhere by remains of these reptiles, and hence the strata containing them may be called the Ceratops beds. They are freshwater or brackish deposits, which form a part of the so-called Laramie, *but are below the uppermost beds referred to that group.* In some places, at least, they rest upon marine beds which contain invertebrate fossils characteristic of the Fox Hills deposits." Italics mine.

If we accept literally Marsh's statement that the Ceratops beds have been traced for eight hundred miles along the eastern flank of the Rocky Mountains, it will be necessary to suppose that he includes in the Ceratops beds not only the beds in Converse Co., Wyoming, but also the *Bison beds* (*Denver beds of Cross*) at Denver, and the *Judith River beds* on the upper Missouri. These are very widely separated localities, and no attempt has ever been made to trace the continuity of the strata from the one to the other, nor is it at all probable that such an attempt would meet with success. Professor Marsh did in the autumn of 1889 spend nearly two days in the Converse Co. locality, and again in 1891 he spent one full day in the same locality; but his time was occupied in visiting a few of the localities in which dinosaur skulls and skeletons and Laramie mammals had been found. No time was taken to determine the upper and lower limits of the beds or to trace the outcrops of the strata. After his visit in 1889 when he spent nearly two days with our party in the Converse Co. locality, he took the train for Denver, and in the company of Mr. George L. Cannon of that city, he spent one-half day examining the *Bison beds* (Denver beds). This constitutes Professor

Marsh's *field work* in the *Ceratops beds.* In a total of three and one-half days field work he seems to have found sufficient time to "carefully explore" the geological deposits of the *Ceratops beds* and to trace them for "eight hundred miles along the eastern flank of the Rocky Mountains," besides making numerous other observations of scientific interest.

Of the many interesting vertebrate fossils described by Professor Marsh from the *Ceratops beds,* those from the Denver locality were secured by Messrs. Cross, Eldridge and Cannon, and those from Wyoming and Montana by the writer or men in his party, with one exception only, namely, the type of *Hadrosaurus breviceps,* which was received at New Haven many years ago, the locality on the label accompanying it being given as Bear Paw Mountains, Montana, which is of course incorrect, it doubtless is from the vicinity of Cow Island. With this one single exception I can confidently state that all the material described by Professor Marsh as from the Laramie or Ceratops beds of Wyoming is, without exception, from Converse Co., and was found within an area not exceeding fifteen miles in width from east to west by thirty miles in length from north to south; and all the material described by him as from Montana, with the one exception mentioned, was found on the Missouri River between the mouth of Arrow Creek, just above Judith River, and the mouth of Cow Creek, some forty-five miles below, and never back farther than ten miles from the Missouri. It will thus be seen that the actually known area of the *Ceratops beds* is indeed very limited, and from these areas we should exclude certainly, the Judith River or upper Missouri and very likely the Black Butte locality in southwestern Wyoming. The beds of the former certainly and those of the latter almost certainly, belong to an older horizon than those of the Denver or Converse Co. localities; the latter may be considered as the typical locality for the Ceratops beds. All of the dinosaurs from the Judith River country are smaller, less specialized forms than those from the Converse Co. and Denver localities, as has already been observed by Marsh.

Marsh's statements that the Ceratops beds are below the uppermost beds referred to the Laramie and that they rest upon

marine beds which contain invertebrate fossils characteristic of the Fox Hills deposits, may well be questioned, especially if we exclude from the *Ceratops beds* the *Judith River beds* and refer them to a lower horizon, retaining for them the name *Judith River beds.* At no place in the Converse Co. region do the *true Ceratops beds*, with remains of horned dinosaurs, rest upon true marine Fox Hills sediments; nor are the Ceratops beds in this region overlaid by strata which could be referred without doubt to the Laramie. The writer has, in a paper published in the American Journal of Science of February, 1893, stated that the *Ceratops beds* rest directly upon the *Fox Hills* series, and has provisionally referred the very similar series of sandstones and shales conformably overlying the *Ceratops beds* to the upper Laramie; but it would doubtless be better to restrict the limits of the Ceratops beds to those strata in which horned dinosaurs occur, and to consider the underlying 400 feet of barren sandstones as the equivalent of the *Judith River beds.* Future investigations will doubtless show that the sandstones, shales and lignites overlying the typical Ceratops beds in Converse Co. should be referred to the *Fort Union beds* and not to the Laramie, as, according to Knowlton, the limited flora sent him now indicates.

The terms Fox Hills and Laramie as now used cannot be taken to represent distinct and different periods of time. for as has been shown by G. M. Dawson, Selwyn and McConnell in the Belly River region in Canada, and frequently observed by the writer on the upper Missouri in Montana, marine beds with typical Fox Hills fossils have been found interstratified with fresh and brackish water beds containing characteristic Laramie fossils, showing conclusively that the two periods were in part at least contemporaneous; the one representing the marine and the other fresh or brackish water forms existing at the same time and in not widely separated regions, these alternations in the nature of the fauna in the same locality having been brought about by successive encroachments and recessions of the sea. It is not at all impossible that in the region of Converse Co., Wyoming, marine conditions prevailed continuously until late in Laramie times, and that during the

time of the deposition of the Laramie beds of the Judith River and Black Butte localities, marine beds with Fox Hills types of fossils were being deposited over the region of what is now eastern and central Wyoming. This would account for the absence in this region, of the lower Laramie, with the smaller and less specialized horned, and other dinosaurs, characteristic of it.

Other Localities for Horned Dinosaurs.

In addition to the localities already mentioned, the writer has seen remains of horned dinosaurs and Hadrosauridæ on the North Platte River opposite the mouth of the Medicine Bow, about 35 miles below Fort Steele, Wyoming; along the eastern flank of the Big Horn Mountains about 40 miles south of Buffalo, Wyoming; on the west side of the Big Horn River between Fort Custer and Custer Station, Montana; and on Willow Creek 13 miles north of Musselshell postoffice in Montana. Another region not examined, but which looked very promising, is near the town of Havre on the Great Northern Railroad just north of the Bear Paw Mountains in northern Montana.

Suggestions to Collectors.

Of all the localities for Laramie dinosaurs and mammals known to the writer, that of Converse Co., Wyoming, is by far the most promising, and the earnest and intelligent collector will there meet with a fair amount of success for many years to come. As will be seen, by reference to the map accompanying this paper, this region is easy of access from the town of Lusk on the F. E. and M. V. R. R. At this station all necessary camp supplies can be obtained. The fossil beds are easily worked, the country being quite open and its surface not disfigured by deep and impassable cañons. There is abundant grass for horses, wood for fuel, and frequent small springs of fairly good water.

Vertebrate fossils are never abundant in the Laramie, but every exposure in this region should be carefully searched and especially the large sandstone concretions which contain many

of the best skulls and skeletons found in these beds. Fossils are found in both the shales and sandstones, but are best preserved in the latter. The small mammals are pretty generally distributed but are never abundant, and on account of their small size are seen with difficulty. They will be most frequently found in what are locally known as "blow outs" and are almost always associated with garpike scales and teeth, and teeth and bones of other fish, crocodiles, lizards and small dinosaurs. These remains are frequently so abundant in "blow outs" as to easily attract attention, and when such a place is found careful search will almost always be rewarded by the discovery of a few jaws and teeth of mammals. In such places the ant hills, which in this region are quite numerous, should be carefully inspected as they will almost always yield a goodly number of mammal teeth. It is well to be provided with a small flour sifter with which to sift the sand contained in these ant hills, thus freeing it of the finer materials and subjecting the coarser material remaining in the sieve to a thorough inspection for mammals. By this method the writer has frequently secured from 200 to 300 teeth and jaws from one ant hill. In localities where these ants have not yet established themselves, but where mammals are found to be fairly abundant it is well to bring a few shovels full of sand with ants from other ant hills which are sure to be found in the vicinity, and plant them on the mammal locality. They will at once establish new colonies and, if visited in succeeding years, will be found to have done efficient service in collecting mammal teeth and other small fossils, together with small gravels, all used in the construction of their future homes. As an instance of this, I will mention that when spending two days in this region in 1893, I introduced a colony of ants in a mammal locality, and on revisiting the same place last season I secured in a short time from the exterior of this one hill 33 mammal teeth.

Another way to secure these small teeth is to transport the material to a small stream and there wash it in a large sieve in the water, the finer material being washed away, but this treatment is too harsh to give the best results, what few jaws there are always being broken to bits.

In the map accompanying this paper the dotted line marked Cer. border, indicates approximately the boundaries of the *Ceratops beds* in Converse Co. The line to the south and east indicating the outcrop of the beds, that to the west the point where they pass under the overlying beds. They extend on uninterruptedly into Weston Co., but have not been worked farther north than Schneider Creek. A working party encamped at the mouth of Schneider Creek on the Cheyenne River, would doubtless meet with much success. The map explains itself; it was drawn by the writer to accompany his paper on the Ceratops beds above referred to, but was not then published. A copy of it was given by him to Professor Marsh and a portion of it redrawn, will appear in the 16th Annual Report of the U. S. Geological Survey accompanying a memoir by Professor Marsh.

Princeton, N. J., Jan. 6, 1896.

RECENT LITERATURE.

Recent Books on Vegetable Pathology.—(1) Kirchner, Dr. Oskar: *Die Krankheiten und Beschädigungen unserer landwirtschaftlichen Kulturpflanzen.* Stuttgart, 1890, pp. VI, 637; (2) Comes, Dr. O.: *Crittogamia Agraria.* Naples, 1891, pp. 600; (3) Ward, Dr. H. Marshall: *Diseases of Plants.* London, (no date), pp. 196; (4) Ludwig, Dr. Friedrich: *Lehrbuch der niederen Kryptogamen.* Stuttgart, 1892, pp. XV, 672; (5) Tubeuf, Dr. Karl Freiherr von: *Pflanzenkrankheiten durch Kryptogame Parasiten verursacht.* Berlin, 1895, pp. XII, 599; (6) Frank, Dr. A. B.: *Die Krankheiten der Pflanzen.* Breslau, 1895–1896, Vol. 1, pp. XII, 344; Vol. II, pp. XI, 574; (7) Prillieux, Ed.: *Maladies des Plantes agricoles et des arbres fruitiers et forestiers causé par des parasites végétaux.* Paris, 1895, Vol. I, pp. XVI, 421.

It is desired only to call attention to these books at this time by means of the briefest mention. Some have been published long enough to enable one to speak freely of their merits and demerits; others are very recent additions to the literature of vegetable pathology and use has not yet demonstrated strong or weak points.

PLATE III.

WESTON Co.
N
Cheyenne River
NATRONA Co.
CONVERSE Co.
DAK.
NEB.
LARAMIE Co.
ALBANY Co.
Antelope Creek
Sand Creek
Stinking Water
Bear Creek
Cheyenne River
Cole Creek
Sage Creek
Box Creek
Lightning Creek
Schneider Creek
Greasewood Creek
Doegie Creek
Cow Creek
Cen. Border
Lance Creek
Lightning Creek
Twenty Mile Creek
L. Lightning Creek
Young Woman Creek
Old Woman Creek
Sage Creek
Buck Creek
Hat Cr. P.O.
Road to Fossil Beds
Indian Creek
Antelope Creek
War Bonnet Creek
Squaw Creek
Brush Creek
Hat Creek
Jim Creek
Platte River
F.E. & M.V.R.R.
Casper
Glen Rock
Deer Creek
Box Elder Creek
Ft. Fetterman
Walker Creek
Shawnee Coal Mine
Shawnee Creek
Shawnee
Douglas
Orin Jc.
Bed Tick Cr.
Duck Creek
Lusk
Manville
Running Water River
Harrison
Lost Creek
Muddy Creek
Willow Creek
Wagonhound Creek
La Bonté Creek
C. & N.R.R.
Platte River
Ft. Laramie
Laramie River
+ indicates localities where Triceratops skulls have been found.
o indicates localities where mammals have been found.
⊙ indicates localities where Hadrosauridae have been found.
MAP OF CONVERSE COUNTY, WYOMING.
SCALE ABOUT 16 MILES TO THE INCH.

Dr. Kirchner's book deals with diseases due both to animal and vegetable parasites. Its statements are reasonably accurate and it is so arranged as to greatly facilitate identification of diseases. No illustrations.

Dr. Comes' book contains quite a full account of some parasitic diseases and brief mention of many others. It was the first book of its kind to pay much attention to bacterial diseases of plants. Its statements are frequently inaccurate and the 17 plates illustrating fungi and fungous diseases are poorly executed and add nothing to the value of the book.

Prof. Ward's little book is by far the best thing in English. It discusses only a few diseases and all of these in a very elementary, popular way, but there are many interesting suggestions, and the facts which are given are usually stated accurately. There are 53 text figures and a brief index. A book of about the same size and style by the same author, on *Timber and Some of its Diseases*, (1889) makes a good companion volume.

Dr. Ludwig's book is uneven in its make up, some parts being quite free from erroneous statements and others, those dealing presumably with the subjects least familiar to the author, needing careful revision. The book certainly deserves a second edition. From the pains taken to say something about everything, it is perhaps more generally useful than any of the preceding or than the following work.

Dr. Tubeuf's book is very attractive. The type is large and clear, and the unhackneyed character of the illustrations, many of which were prepared expressly for this work, is especially commendable. The treatment of certain subjects indicates that the author depended upon imperfect reviews rather than on the original papers, e. g., Wakker's bacterial disease of hyacinths, and Mayer's mosaic disease of tobacco; but the book as a whole has not been read carefully enough to warrant any extended criticism.

Dr. Frank's book is the second revised edition of his well known handbook, *Die Krankheiten der Pflanzen*, published in 1880, and now sadly out of date. Much new matter has been added and an earnest effort made to bring the subject up to date. This has succeeded as well, perhaps, as the rapidly growing state of the science will admit. The first volume deals with non-parasitic diseases; the second with fungous parasites. Most of the figures appear to be old, and the letter press is indifferent.

Dr. Prillieux's book is attractive in appearance, but some of it is sketchy and rather unsatisfactory, and due credit is not always given.

Quite often the reader finds himself wishing the author had stated some matter exactly rather than vaguely, e. g., germination of the oospores of *Plasmopara viticola.* Prillieux is probably right in maintaining that Viala has not satisfactorily determined the aetiology of Brunnisure and the California vine disease, the microscopic appearances ascribed to a Plasmodiophora being quite as likely due to the effect of strong reagents on the protoplasm of the cell. Some of the figures in this book are excellent, others are very poor. There is no index.

It is to be hoped that Dr. Sorauer will now bring out another edition of his *Handbuch der Pflanzenkrankheiten*, or at least of the 2nd volume on parasitic plants which was issued in 1886 and needs revision badly. All of these books are useful to American students, and should certainly find place on the book shelves of every vegetable pathologist. It would seem that the time is not ripe for the appearance of standard American works on this subject. There is, however, great activity in the study of plant diseases in this country, and we may look for a crop of them within the next decade.—ERWIN F. SMITH.

The Iowa University Bahama Expedition.[1]—The history of an educational and scientific experiment is given Mr. C. C. Nutting in this octavo volume of 251 pages. It is published as Bulletins Nos. 1 and 2, Vol. III, of the laboratories of Natural History of the Iowa State University. The zoology of the region visited is treated of in a general way with a view to giving an idea of the facies of the collections from the several localities. The marine and land invertebrata are treated of quite fully, but none of the vertebrates receive much attention excepting the birds. The beauties of marine life are graphically described, and a considerable number of illustrations add to the general excellence of the get up of the book. An appendix gives a list of commissary stores actually used during the expedition.

Mr. Nutting, in summing up the results of the expedition, draws attention to the fact that this enterprise demonstrates the practicability of accomplishing such results at a cost which is merely nominal.

The Shrews of North America.[2]—The tenth number in the North American Fauna series published by the U. S. Department of Agriculture, contains three papers on the Shrews: A revision of the genera Blarina and Notiosorex by Dr. C. H. Merriam, a synopsis of the

[1] The Bahama Expedition. Bulls. Nos. 1 and 2, Vol. III, Laboratories Nat. Hist. Iowa State Univ. Iowa City 1895.

[2] North American Fauna No. 10, Washington, 1895. Comprising papers by C. Hart Merriam and G. S. Miller, Jr.

genus Sorex by the same author, and a discussion of the long-tailed Shrews of eastern United States by G. S. Miller, Jr.

In regard to the short tailed Shrews of the genus Blarina, Dr. Merriam states that up to the present time 8 valid species have been described from the United States, 2 from Mexico, 1 from Guatemala and 2 from Costa Rica. Twelve new forms are here added, 3 from the eastern United States and 9 from Mexico, making 20 members of the genus now known. The type localities are given and the geographical distribution. A complete synonomy accompanies each description.

Dr. Merriam's second paper is a synopsis of the species of Sorex, and is based on an examination of 1200 specimens. In this material were found 20 new forms which are here described. In this paper, as in the first, careful attention has been given to the synonomy.

The only genera of Soricidae included in this monograph by Dr. Merriam are Blarina Gray, Notiosorex Baird and Sorex Linn.

Mr. Miller's contribution is a study of the long tailed Shrews of the eastern United States. The author gives in detail the history of each species. The descriptions include the type locality, geographic distribution, and detailed information under the head of general remarks.

Figures of all the species described are given on 12 page plates, and they are of excellent quality. The monographs are the most important contributions to the subject that have been made, and are indispensable to the student of N. American mammalia.

Iowa Geological Survey, Vol. III.[3]—A quarto volume containing the several reports of the geological corps, with accompanying papers of the geology of special formations and areas. The work in the southwest half of the State was done under the immediate supervision of Dr. Keyes who contributes three papers on the geology of that section, and also one on the glacial scorings in Iowa. Mr. Calvin discusses the composition and origin of the Iowa Chalk. The Paleozoic strata in the northeastern part of the State, and certain Carboniferous and Devonian outliers in the eastern region are reported upon by Mr. Norton. The Cretaceous deposits of the Sioux Valley by Mr. Bain and certain buried River Channels by Mr. Gordon. The illustrations include 37 plates, a number of maps, and 34 figures in the text. We are glad to learn that the survey is in a prosperous condition, and hope that its work will be appreciated at its true worth by the State authorities.

[3] Iowa Geological Survey, Vol. III. Second Annual Report, 1895, with accompanying papers. Des Moines, 1895.

Duration of Niagara Falls and History of the Great Lakes.[4]—This work contains the researches of the author which have been published in America and Europe, on the Origin of the Great Lake Basins; Changes of Continental Altitudes; Deformation of Beaches; Glacial Dams; Births of Lakes Ontariò, Erie, Huron, etc.; Changes of River Courses; and the History and Duration of Niagara Falls. It is one of the most important works on geological science that has been produced in this or any other country as an original research. It furnishes a standard of estimation of postglacial history for this continent, which must always be referred to in all questions relating to the antiquity of man, as well as those relating to the present distribution of land and water.

The text is fully illustrated with maps, section drawings, etc. One of the fine page plates which accompany the work is a reproduction from a camera obscura drawing made by Henry Ransford in 1832, the oldest accurate picture of the Falls known to the author.

The author estimates that the period which has elapsed since the falls were at Lake Ontario amounts to 32000 years.

Korean Games.[5]—In pursuance of a theory that games must be regarded as survivals from primitive conditions, under which they originated in magical rites and chiefly as a means of divination, Mr. Stewart Culin has made an extensive study of the games of Korea. He finds that there were two principal systems of divination in Eastern Asia from which games arose, in both of which the arrow or its substitute was employed as the implement of magic. Of the 97 games described in his book, 23 are directly connected with some such use of the arrow. A large number of the other games described consist of athletic sports ceremonially practiced in the sacred pavilions of Korea, and like the divatory tugofwar, still retain traces of their primeval divinatory character.

The illustrations are almost entirely by native artists, and they give the book a value altogether unique. They comprise 22 colored plates and 135 figures in the text. The subject is a very curious one, and as treated by Mr. Culin, it becomes an important guide to the history of human migrations and human thought.

[4] The Duration of Niagara Falls and the History of the Great Lakes. By J. W. Spencer. New York, Humboldt Publishing Co.

[5] Korean Games. With Notes on the Corresponding Games of China and Japan. By Stewart Culin. Philadelphia, 1895.

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General Notes.

PETROGRAPHY.[1]

Igneous Rocks of St. John, N. B.—W. N. Mathew has continued his work on the igneous rocks of St. John, N. B.,[2] contributing in a recent article an account of the effusive and dyke rocks of the region. All the rocks described are believed to be pre-Cambrian in age. They embrace quartz-porphyries, felsites, porphyries, diabases and feldspar-porphyrites among the effusive rocks, and diorite-porphyrites, diabases and augite-porphyrites among the dyke forms. In some of the quartz-porphyries perlitic cracks may still be recognized, and in the felsite porphyries some spherulites. Tuffs of all the effusives are abundant. A soda granite with augite and green hornblende and probably a little glaucophane was also met with. It is intrusive, and has a composition represented by the figures:

SiO_2	TiO_2	Al_2O_3	Fe_2O_3	FeO	MnO	CaO	MgO	Na_2O	K_2O	CO_2	Loss
64.86	.70	15.02	5.53	1.01	.18	2.61	1.42	3.92	2.37	.55	1.73

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Trans. N. Y. Acad. Science, XIV, p. 187.

The diorite-porphyrite has a groundmass of idiomorphic hornblende, lathshaped feldspars and some interstitial quartz, with phenocrysts of the same minerals, but principally of feldspar. Among the diabases is a quartzose variety.

Eruptive Rocks from Montana.—Among some specimens of eruptive rocks obtained from Gallatin, Jefferson and Madison Counties, Montana, Merrill[3] finds basalts, andesites, lamprophyres, syenites, porphyrites, wehrlites, harzburgites and websterites, some of which possess peculiar characteristics. A hornblende andesite, for instance, contains large corroded brickred pleochroic apatite crystals, whose color is due to innumerable inclusions scattered through them. The groundmass of some of the basalts has a spherulitic structure. The wehrlite is a holocrystalline aggregate of pale green diallage, reddish brown biotite, colorless olivine and a few patches of plagioclase. Its structure is cataclastic or granulitic, the larger crystals being surrounded by an aggregate of smaller ones. The websterite consists of green diallage and colorless enstatite with included foliae of mica and occasional interstitial areas of feldspar, and is thus related to gabbro. Some of the lamprophyres are composed of groups of polysomatic olivines or of olivine and augite in a scaly granular groundmass of lighter colored minerals, through which are scattered small flakes of brown biotite and tiny augite microlites. This structure is accounted for on the supposition that the granular groups of olivine and of olivine and augite belong to an older series of crystalline products than those of the groundmass.

Porphyrites and the Porphyritic Structure.—In a general account of the laccolitic mountains of Colorado, Utah and Arizona, Cross[4] gives a brief synopsis of the characteristics of the rocks that constitute their cores. These rocks comprise augite, hornblende and hornblende mica-porphyrites, diorites and quartz-porphyrites. All contain phenocrysts of plagioclase and of the iron bearing silicates, with the feldspars largely predominating. These upon separating left for consolidation into the groundmass a magma which upon crystallization yielded a granular aggregate consisting largely of quartz and orthoclase. No pressure effects were seen in any of the sections studied. All are porphyritic with a granular groundmass, which differs in the different rocks, principally in the proportion of its constituents. The porphyritic structure as defined by the author is not the result of the recur-

[3] Proc. U. S. Nat. Museum, XVII, p. 637.

[4] 14th Ann. Rep. U. S. Geol. Survey.

rence of crystallization, producing several generations of crystals, but it is a structure exhibiting contrasts in the size and form of the component crystals of a rock, resulting from the differences in conditions under which the different minerals crystallized.

Granophyre of Carrock Fell, England.—In the Carrock Fell district is a red granophyre closely associated with the gabbros. This rock has recently been studied by Harker,[5] who had previously investigated the gabbros. The normal type of the granophyre is an augitic variety in which the augite occurs as a deep green species which is idiomorphic toward the feldspars. Oligoclase is also present as idiomorphic crystals in a reddish quartz-feldspar groundmass with the typical granophyric structure. The composition of the rocks is represented as follows :

SiO_2	Al_2O_3	Fe_2O_3	MgO	CaO	Na_2O	K_2O	Loss	Total
71.60	13.60	2.40	.21	2.30	5.55	3.53	.70	= 99.89

As the rock approaches the gabbro it becomes less acid and the proportion of augite in it increases. This is the lower portion of the mass as it was originally intruded. Its more basic nature as compared with the rest of the rock is explained as due to the absorption of parts of the gabbro with which the granophyre is in contact.

The same author[6] also records the existence of a greisen, which is a phase of the well known Skiddau granite. The greisen consists essentially of quartz and muscovite, but remnants of orthoclose are still to be detected in it. The mica is regarded as having been derived largely from the feldspar.

Sheet and Neck Basalts in the Lausitz.—The basalts of the neighborhood of Seifeirnersdorf and Warnsdorf in the Lausitz, Saxony, occurs in sheets according to Hazard,[7] and in volcanic rocks. The sheet rocks are nepheline basalts, nepheline basanites and feldspathic glass basalts. The neck forms are hornblende basalts, sometimes with and sometimes without nepheline. The constituents of all are magnetite, apatite, augite, biotite, nepheline and glass in varying quantities, with feldspar, olivine and hornblende in different phases. Sometimes the mineral nepheline is absent, but this happens mainly in the glassy varieties, where its components are to be found in the glassy base. There are intermediate varieties between the hornblende and the oli-

[5] Quart. Journ. Geol. Soc., 1895, p. 125.

[6] Ibid, p. 139.

[7] Min. u. Petrog. Mitth., XIV, p. 297.

vine basalts corresponding to geological masses intermediate in characteristics between volcanic sheets and necks. In many of the neck rocks the hornblende is seen to have been partially resorbed and changed to augite. The continuation of the resorbtive process until every trace of the hornblende was dissolved, may account for the absence of the mineral in the sheet rocks.

Petrographical Notes.—In an article whose aim is to call forth more accurate determinations of the feldspars in volcanic rocks, and one which gives a practical method for making this determination, Fouqué[8] has described briefly the volcanic rocks of the Upper Auvergne, the acid volcanics of the Isle of Milo and the most important rocks in the Peleponeses and in Santorin. Among the varieties described are doleritic basalts, andesitic basalts, labradorites, andesites, obsidians, trachyte andesites, phonolites, andesitic diabases, rhyolites, dacites and normal basalts. The labradorites are composed largely of microlites of labradorite with a few augites and tiny crystals of olivine in an altered glassy base. In all these cases the author has shown that the rocks contain several different feldspars at the same time, and in each case he has determined their nature. The method made use of in the determination is based on the observation of extinction angles in plates cut perpendicular to the bisectrices.

In a well written article on complementary rocks and radial dykes Pirsson[9] suggests the name of oxyphyre for the acid complementary rock, corresponding to the term lamprophyre for the basic forms. He also calls attention to the fact that the dykes radiating from eruptive centers are usually filled with younger material than that which composes the core at the center. The dykes cutting the central mass will generally be oxyphyres and the more distant ones lamprophyres.

Cordierite gneisses are reported by Katzer[10] from Deutshbrod and Humpolitz in Bohemia, where they are intruded by granite veins, and where masses of them are occasionally completely surrounded by granitic material.

In the examination of a large series of granites and gneisses from the borders of the White Sea, Federow[11] discovered that garnet is present in large quantities when plagioclase is absent and vice versa.

In a general article on the Catoctin belt in Maryland and Virginia,

[8] Bull. Soc. Franc. d. Min., XVII, p. 429.

[9] Amer. Journ. Sci., 1895, p. 116.

[10] Min. u. Petrog. Mitth., XIV, p. 483.

[11] Ibid, p 550.

Keith[12] gives very brief descriptions of the granites, quartz porphyries, andesites and the Catoctin schist of the region. The last named rock is apparently a sheared basic volcanic. All the rocks present evidence of having suffered pressure metamorphism.

GEOLOGY AND PALEONTOLOGY.

Notes on the Fossil Mammalia of Europe.—I, Comparison of the American and European forms of Hyracotherium.—Historically speaking Hyracotherium is one of the oldest of known fossil Perissodactyla, and it is of importance phylogenetically to compare the representatives of this genus in Europe with those of America, in order to acquire an exact knowledge as to the evolution of the molar cusps of the New and Old World species. My attention was called to this subject on account of having studied *Euprotogonia* of the Puerco, a genus which as well known, is considered to have one of the most primitive types of Ungulate molars.

The importance of having accurate drawings of the teeth of fossil mammals is nowhere better illustrated than in *Hyracotherium.* In the case of the enlarged drawing of the teeth[1] of *H.* (=*Pliolophus*) *vulpiceps* which has been copied extensively in works on vertebrate palæontology, we obtain quite an erroneous idea of the exact form of the molar cusps.

Kowalevsky[2] in his great work on "*Anthracotherium*" figures some of the molars of the type of *Hyracotherium* namely: *H. leporinum*, and I should judge from his description that he had studied Owen's type in London. However, his criticism of Owen's drawing of the type of *Hyracotherium* is very accurate, and as Kowalevsky remarks, Owen's figures gives one the idea that the teeth of the type are strictly bunodont, whereas they are really transitional in structure between a real bunodont type, such as *Euprotogonia* and a truly lophodont form like *Systemodon.*

[12] 14th Ann. Rep. U. S. Geol. Survey, p. 285.

[1] Jour. of the Geog. Soc. of London, 1858, p. 54.

[2] Monographie der Gattung Anthracotherium, p. 205.

On my recent visit to London[3] I took the opportunity to examine both of the types of *Hyracotherium* (*H. leporinum* and *H.* (=*Pliolophus*) *vulpiceps*). In general the external cusps of the superior molars in both forms is lenticular in section, being considerably drawn out anteroposteriorly, and the intermediate tubercles are well extended transversely. In the drawing of the type specimen of *H* (=*P.*) *vulpiceps* the molars are represented as greatly enlarged, and their internal portions shown as complete. In the original specimen the teeth are damaged internally, and it is with some diffiulty that the form of the cusps can be made out. However, I am satisfied that the internal cusps are not really bunodont as shown in the plate; but like the American forms of *Hyracotherium* these cusps are extended transversely and form by wear slight ridges with the intermediate tubercles. In comparing the upper molars of both types of *Hyracotherium* of England with those of the Wasatch of America, I find them to be in exactly the same stage of evolution as to the form of the cusps.

Lydekker[4] speaks of the posterior transverse crest of the upper molars in the type of *H.* (=*P.*) *vulpiceps* as not being represented as sufficently well developed in the plate, but this crest on the last molar is drawn correctly, and on the other two molars it is as nearly as well developed. A comparison of the upper molars in both types of *Hyracotherium* with those of *Euprotogonia*, reveals the fact that the form of the cusps in the former genus has undergone a progressive change; and this is seen especially on the last upper molar which is quadritubercular with a large development of the metaloph, whereas in *Euprotogonia* the last upper molar is tritubercular. Again the third upper premolar in the English types of *Hyracotherium* is tritubercular whereas in *Euprotogonia*, this tooth has only one external cusp.

The species of *H.* (=*Pliolophus*) *vulpiceps* is of importance, as in this specimen we have both upper and lower teeth belonging to the same individual. I should like to particularly emphasize the point that the last lower premolar in the type of *Pliolophus* is *simpler in structure than the first true molar, the posterointernal cusp being absent.* This character distinguishes this type from some forms of the American Wasatch which have been referred to *Pachynolophus*.

[3] I wish to express here my thanks for the privileges I enjoyed in examining specimens in the British Natural History Museum, and especially to thank Sir W. H. Flower for his kindness. I am also indebted to him for having been able to visit the Royal College of Surgeons. Mr. C. W. Andrews of the Geological Department. I am also much indebted for his many courtesies.

[4] Catalogue of Fossil Mammals in British Museum. p. 11.

The question now arises what is *Pachynolophus*, or in other words what is the exact generic definition of this genus, and does it really occur in the Wasatch of America. It is usually stated that *Pachynolophus* is separated from *Hyracotherium* by the fact, that the last premolar is molariform but as far as I have been able to investigate, this definition will apply to only one European species, namely: *Pachynolophus* (=*Hyracotherium*) *siderolithicus*, and even in this species the last upper premolar exhibits a good deal of variation in its structure. Rütimeyer[5] figures Pictet's species, *P. siderolithicus*, which in this specimen has the last upper premolar molariform, but in the same plate (fig. 21) is given another last upper premolar, which Rütimeyer referred to the same species. This tooth is tritubercular, or simpler in structure than the true molars.

Among the specimens of *Pachynolophus* in the collection of the Jardin des Plantes, Paris, there is a series of loose teeth from the Siderolithic du Mauremont, which are of considerable interest as they were studied by Kowalevsky, and referred by him to *P. siderolithicus*. This series contains at least one last upper premolar, and it has exactly the same character as that figured by Rütimeyer, in other words this is another case in which this tooth in *P. siderolithicus* is simple in structure. In *Pachynolophus duvalii* as figured by Rütimeyer, the last upper premolar is tritubercular, and this tooth has the same structure in *P. desmarestii*. In *Pachynolophus cessarasicus* figured by Filhol[6] from the Middle Eocene of Céssai, the last upper premolar has only one internal cone and the two transverse ridges diverging from it are well developed. The length of the skull in this species is about one-third greater than that of *Hyracotherium venticolum* of the Wasatch.

Numerous species of *Pachynolophus* occur in the different horizons of the Eocene of France, but in nearly all cases, they are represented either by upper or lower molars which were not found together. I believe the English *Hyracotherium leporinum* is the only known form in Europe in which both upper and lower molars were found associated, and actually belong to the same individual.

As is well known it is the last lower premolar which first becomes molariform, consequently if we find forms in which this tooth is simpler in structure than the molars, we can safely conclude that none of the superior premolars are molariform. In a jaw referred to *Pachynolophus* in the collection of the Jardin des Plantes from Phosphorites, containing all the lower premolars, the last tooth of this series is not molari-

[5] Eocäne Sangethiere Welt von Egerkingen, pl. III, figs. 18–21.

[6] Vertébres Fossiles d'Issel Mém. Soc. Geog. de France, 1888, pl. XX, fig. 13.

form. As the Phosphorites are considered to represent the top of the Eocene, we should certainly expect to find in any species of *Pachynolophus* from this formation the last premolar as complex in structure as the molars. In this jaw, however, the last premolar is not molariform and the collection contains a crushed skull of the same species of *Pachynolophus* in which all the superior premolars are simpler in structure than the true molars.

I can find no good generic differences based on tooth structure separating *Propalæotherium* from *Pachynolophus*, and shall consider the former genus as a synonym of the latter in this paper. In *Propalæotherium* the species are much larger than in those of *Pachynolophus*, but certainly size alone can not be considered as of value in generic definitions. In all of the species included in *Propalæotherium*, the premolars are simpler in structure than the true molars.

I believe, however, if we divide the various known species of the *Hyracotheriinæ* into genera according to the complication of the premolars, that we shall be adopting an artificial character, and as shown above. Of all the known forms of *Pachynolophus*, *P. siderolithicus* is the only one in which the last upper premolars is molariform and even this species shows considerable variation in this respect. I conclude then that the only natural classification of these forms is a careful analysis of the form of the molar cusps, and to group the species into genera according to the development of the same; I refer here especially to the European forms of *Hyracotherium* and *Pachynolophus*.

Having thus attempted to show that in nearly all the European species of *Pachynolophus* the last premolar is simpler in structure than any of the true molars, I come to consider what are the generic differences separating *Hyracotherium* from *Pachynolophus*. Kowalevsky studied *Pachynolophus siderolithicus*, and if we compare the molars of this species with those of *Hyracotherium angustidens* from the Wasatch of America, we observe at once that the external cusp of the upper molars in the latter species are nearly round in section, and they are scarcely at all flattened. There is no mesostyle and the height of the crown is very low or strongly brachydont. In *P. siderolithicus* the ectoloph is considerably lengthened from above downwards, and the external cusps are strongly flattened, with a prominent mesostyle. In all the species of *Pachynolophus* which I have studied the molar crowns are higher than those of *Hyracotherium*, and in all the mesostyle is strongly developed. The latter characters demonstrate that the molars of *Pachynolophus* have reached a higher stage of evolution than those of *Hyracotherium*, and this transformation in the form of the molar cusps

points to a still higher differentiation so characteristic of the later genera of the Equine series. The characters above enumerated as distinguishing the molars of *Pachynolophus* from those of *Hyracotherium*, I think should be considered as generic. At least they are the only valid ones which I can discover at present.

Species of Perissodactyle Ungulatus allied to *Hyracotherium* occur in the Wasatch of America, which have been referred to *Pachynolophus*. In these forms the last inferior premolar is truly molariform, but the superior molars are of the primitive type namely; with low crowns and nearly bunodont external cusps. As this a combination of characters, which as far as known does not occur in the true *Pachynolophus* of Europe, I think accordingly that these species should be placed in a different genus from *Pachynolophus*. Prof. E. D. Cope has shown that the generic name *Orotherium* Marsh, has been anticipated by *Orotherium* Aymard, consequently I believe that the name *Orohippus* should be reinstated, and applied to those species of *Hyracotherium* which have the last lower premolar truly molariform in structure. I have already stated that in the type specimen of *Hyracotherium* (=*Pliolophus*) *vulpiceps* the last lower premolar is simpler in structure than in any of the true molars. Accordingly those species with the complex lower premolar represent a true generic stage, and as is already shown they differ from the true *Pachynolophus*.—CHARLES EARLE, *Laboratoire de Palæontologie Jardin des Plantes, Paris, Dec.* 20, 1895.

The Glossopteris Flora in Argentina.—A collection of fossil plants from Bajo de Velis, a league from the entrance to the Cantana valley in Argentina, has enabled Dr. F. Kurtz to establish the age of the fossiliferous shales of that region. The author gives tabulated comparisons of the flora of the Bajo de Velis beds with similar floras found at the Cape of Good Hope (Ekka-Kimberly beds), in peninsular India (Kaharbari beds), in New South Wales (Newcastle beds) and in Tasmania (Mersey Coalfield). From these tables it is seen than the specimens found at Bajo de Velis are most nearly related to those of the lower Gondwanas of India. Dr. Kurtz accordingly corrrelates the fossiliferous shales of Bajo de Velis with the lower Gondwanas of India, and agrees with the paleophytologist, O. Feistmantel, in assigning these beds to the Permian age.

Up to date but three rock formations in Argentina have yielded fossil plants: Retamito in San Juan, which has been shown by Dr. Szajnocha to be Lower Carboniferous; Bajo de Velis which is Permian; and

a series of beds in Mendoza, San Juan and La Rioja determined by Prof. Geinitz as Rhætic.

These data are commented on by the Director of the Geological Survey of India to the effect that one of the chief points of interest in connection with the discovery of Gondwana plants in Argentina lies in the fact that we have an unquestionable lower carboniferous series (Retamito) in the neighborhood of which (and probably unconformably to it) a series of beds is found, which contains well known Lower Gondwana species of plants, thereby limiting the geological range of the lowest beds of it, at all events to upper Carboniferous at most, which is a further confirmation of the views generally adopted by the Geological Survey of India. The genus Glossopteris proper is wanting, but the other genera characteristic of that flora are present. (Rec. Geol. Surv. Ind., Vol. XXVIII, 1895.)

Geological News.—PALEOZOIC.—From a petrographic study of the igneous rocks near St. John, N. B., Mr. W. D. Matthew classifies the Pre-Cambrian of that region as follows: A. Laurentian composed of (1) Portland group and (2) Intrusive granite, B. Huronian composed of (3) Coldbrook group, of volcanic rocks, (4) Coastal group, of volcanic and sedimentary rocks, (5) Etcheminian or Basal Series, of sedimentary rocks, and (6) Kingston group, of metamorphosed volcanics. (New York Acad. Sci. XIV, 1895.)

MESOZOIC.—In studying the fossils obtained by M. Gautier from Madagascar, M. Boule comes to the conclusion that the Jurassic deposits of eastern Africa and those of the western slopes of Madagascar appear to have been laid down in a great interior sea, an Ethiopian Mediterranean, which was separated from the Pacific by an Indo-Madagascar peninsula.

Furthermore, that during the Upper Cretaceous there was land communication between the African continent, Madagascar and Hindustan. (Bull. Mus. d'Hist. Nat. Paris, 1895.)

According to R. W. Ells, the whole range of North Mountain, which cuts off the valleys of Cornwallis and Annapolis rivers from the Bay of Fundy, is an overflow of igneus rock which has issued through a line of fissure transversing the red Triassic beds, and is, therefore more recent than the latter. At several places the trap is overlaid by newer sedimentary beds of limestones and shales. No fossils have as yet been found in these sedimentary strata. The author calls attention to their importance and the desirability of a thorough exploration in order to determine their age since they represent the highest group of stratified sedimentary rocks in Eastern Canada. (Trans. Nova Scotian Inst. Sci. Halifax, Vol. I, 1894, p. 416.)

Two new species of Fishes from the Rolling Downs Formation (Lower Cretaceous) of Queensland are described by A. S. Woodward. They represent species of the genera Portheus and Cladocyclus, to which he gives the names *australis* and *sweetii* respectively. This discovery of these fossils is of considerable interest, since with the exception of a few Selachian teeth and vertebræ, and a fine species of Belonostomus, no cretaceous ichthyolites of importance have hitherto been described from this colony. (Ann. Mag. Nat. Hist. (6) XIV, 1894, p. 444.)

Cenozoic.—Fossil Ants are reported from the Bembridge limestone (Eocene) of the Isle of Wight. They are referred by P. B. Brodie to the genera Formica, Myrmica and Camponotus, and some others not yet described. The first two genera have also been found in the Baltic Amber. (Nature, 1895, p. 570.)

The Champlain epoch is correlated by Prof. Hitchcock with the Mecklenburg stage of Geikie. Both have the characteristic marine mollusca fauna, the Arctic flora (*Yoldia* beds of the Baltic) and best illustrate the isobases of De Geer. (Bull. Geol. Soc. Amer., Vol. 7, 1895.)

VEGETABLE PHYSIOLOGY.[1]

Smut Fungi by Oscar Brefeld.—At last we have in two big quartos, with numerous plates, the long promised volumes on the smut fungi. The work which is here completed was begun more than 12 years ago. The earlier experiments were gathered together and published in 1883 in a volume of 220 pages with numerous plates under the title of *Die Brandpilze I*, forming Heft V of Dr. Brefeld's Untersuchungen, the most important and revolutionary portion of this volume being the demonstration that the smut fungi, a goodly number at least and presumably all of them, although previously supposed to be strictly parasitic were capable of growing saprophytically and of multiplying indefinitely in dung in the form of sprout conidia, closely resembling yeasts, if not identical with many forms previously referred to this group. Some years later in an address before the agricultural club of Berlin, Dr. Brefeld communicated the most important results of his

[1] This department is edited by Erwin F. Smith, Department of Agriculture, Washington, D. C.

magnificent infection experiments, but now for the first time we have full details of all the laboratory and field investigations. In the limits of this review it will be possible to notice only the first of these two volumes. This forms Heft XI of the Untersuchungen and is entitled *Die Brandpilze II.* It deals principally with infection experiments and gives in full the results obtained with *Ustilago Carbo* on oats, *U. cruenta* on sorghum, and *U. maydis* on maize. These experiments were carried on through a period of four years with striking results and in case of corn, with most unexpected ones. Space forbids entering into much detail. Those who wish for details will naturally consult the volume itself. Suffice to say that the infective material consisted of the yeast-like conidia propagated in nutrient solutions made from fresh horse dung.

In case of oats the best results from direct infection were 17 to 20 per cent. of smutty plants, obtained by spraying during the earliest stage of germination. Infections made when the embryo was one cm. long gave only 7 to 10 per cent of smutty plants; when it was 2 cm. long (500 plants), only 2 per cent became smutty. When the plumule had pushed through the enfolding sheath scarcely any of the plants could be infected, 200 seedlings in this stage yielding only 1 per cent of smutty plants and 200 more remaining entirely free. The infections took place through the young axis and also through the sheathing leaf so that both Wolff and Kühn were right, but a majority of the infections were through the young axis. In a second series of experiments garden earth was sprayed with the smut conidia and two days later oats were planted 1 cm. deep and subsequently transplanted to the open field: 300 of these seedlings yielded 5 per cent of smutty plants, and 300 more, 4 per cent., i. e. a much smaller per cent than was anticipated. In a third series fresh horse dung was mixed with garden earth which was then abundantly impregnated with the smut conidia. Three days later oats which had been soaked but were not yet germinated were planted in this soil at a depth of scarcely 1 cm. These seedlings were divided into two lots, 300 were kept for a time in the laboratory at a temperature of over 15°C., and 300 were placed in the cellar where the temperature did not exceed 7°C. Of the 300 kept in the laboratory 27 to 30 per cent finally became smutty; of those kept in the cellar, where germination proceeded more slowly, 40 to 46 per cent became smutty. This shows clearly that fresh horse dung greatly favors the development of smut and that weather which retards germination is also favorable. In the fourth series of experiments the infectious material was derived from conidia cultivated for a long time arti-

ficially, a few of the spores being transferred to a fresh nutrient solution every four days. The first trial (500 seedlings) was with conidia which had been cultivated in this manner for six months. These seedlings yielded from 7 to 10 per cent of smutty plants. The second trial was with conidia which had been cultivated for a year. This experiment was almost wholly negative, 300 of the seedlings yielded no smutty plants and 200 more gave only 1 per cent. The explanation was not far to seek since at the end of this period the conidia had almost wholly lost the ability to send out germtubes and along with it the power to infect the plants. Microscopic examination showed that the germtubes can penetrate into any part of the young seedling but this does not necessarily mean infection. The latter takes place only when the smut hyphæ are able to reach that part of the plant where the smut beds form. In all of these experiments the smut germs penetrated the young seedlings but the smut beds appeared only in the floral organs, some months intervening between the entrance of the fungus and the appearance of the smut in a totally different part of the plant. Those germtubes which enter the plant and fail to reach the incipient ovaries become enclosed in the mature tissues of the host plant and are incapable of further growth and this frequently occurs even in young seedlings.

The infections obtained with the big sorghum plant are even more interesting. Nearly all of the first series of infections were destroyed by a hail storm, but of the 32 plants which escaped 12 became smutty. The seedlings of the second series were infected indoor in March and set out the first of May. The plants grew luxuriantly and by the middle of August had reached a height of 5 to 7 feet. The first smutty panicle appeared August 16 and for some time thereafter it appeared as if all of the plants would be smutty, the infected panicles developing first. Finally sound ones began to appear. In the end there were 158 smutty plants out of 274. A third series of experiments was instituted to determine in what stage of germination the sorghum plant is most susceptible: 252 seedlings sprayed in the earliest stage of germination, gave 180 smutty plants. "The development of the smut in the earliest and strongest plants, which reached a height of 8 feet, was striking. The big panicles were attacked in toto and projected out of the luxuriant green foliage like black brooms." There can be no doubt that infection stimulates the growth of the plant. Older seedlings yielded less striking results: 150 which were infected when the embryo was a centimeter long, gave only 24 smutty plants; 190 infected when the embryo was 1½ cm. long gave 12 smutty panicles; 221 infec-

ted when the plumule had begun to push through the sheath gave 5 smutty plants; finally, 150 infected when the plumule had pushed through the sheath about 1 cm., remained entirely free from smut. Microscopic examinations made a few days after the conidia were sprayed on the seedlings showed that germtube penetrations were very common in that experiment which yielded over 70 per cent of smutty plants, infrequent in those which yielded only a small per cent of smutty plants, and altogether absent in the plants which remained entirely free from smut. As in oats, the smut was confined exclusively to the panicle, and the bulk of the infections took place during the earliest stage of germination, the tissues of the growing seedling very soon becoming immune.

The results with maize were very surprising since they developed three wholly unexpected facts, viz.: (1) The germtubes are capable of penetrating any young rapidly growing part of the plant; (2) The growth of the fungous hypha which has gained entrance into the plant is narrowly localized, the sporebeds developing in situ; (3) There is no period of rest, the smut beds developing immediately, i. e. within two or three weeks of the date of infection. Previous to these experiments it was supposed that corn smut entered the plant when it was a seedling and followed the same law of development as oat smut. In the first series of experiments, which proceeded upon this supposition, the smut conidia were sprayed upon 200 seedlings in the earliest stage of germination; upon 100 which were a little older; upon 100 still further advanced; and, finally, upon 100 when the plumule was pushing through the sheath. This work was done in the laboratory and after 14 days the plants were set out in the garden. Contrary to all expectation, very few penetrations could be found even by the most careful microscopic examinations, and these were confined to the root node, none being found upon the sheath,—everywhere over the surface crept the germtubes without being able to enter. These plants were under daily observation and after 10 to 14 days a few lagged behind the rest in growth, and on being pulled up smut pustules were found on the axis a little above the root node. Of the whole 500 seedlings, only a few became smutty, viz., 4 per cent in the youngest and 1 to 2 per cent in the older seedlings. In all of them the smut pustules appeared on exactly the spot where the germtubes had entered the plant and within three weeks of the date of infection. All the other plants grew to maturity and remained free from smut. Similar results were obtained from an experiment in which soaked, ungerminated kernels of corn were planted in a dunged soil which had been

abundantly infected with smut conidia. Of the 50 plants thus treated one died at the end of 4 weeks from a smut pustule on the axis, and the rest developed without any appearance of smut. Another experiment was undertaken with 150 seedlings still further advanced, the conidia being sprayed upon them, but this also gave negative results. No germtube penetrations could be found and no smut appeared upon any of the plants. These results led to a good deal of speculation and finally to the following experiments: The first of these was with plants a foot high, having a well developed cornucopia-like summit formed by the closely wrapped bases of the large outer leaves. One hundred plants were selected and into these cornucopias a nutrient solution containing smut conidia was injected. They were covered with straw matting five days to keep off rain and then freely exposed. On the tenth day, as growth continued and the infected parts were pushed up into sight, there was a changed appearance. The parts of the leaves touched by the infectious fluid were paler than the upper noninfected parts and suggested chlorosis. This appearance was visible in different degrees on all the infected plants? Already there were slight appearances of pustules and within a day or two they became very distinct, finally covering the whole infected surface with a smutty crust. Scarcely one of the male inflorescences escaped and the axis between the leaves was also smutty in so far as the infective material could reach it. Not one of the hundred plants escaped infection, the youngest suffering most. For the next experiment younger plants were selected, i. e. those about six inches inches high. In many of these the cornucopia was not well developed and allowed the infectious fluid to run out and waste and the infection miscarried. All, however, that were large enough to retain the conidia were killed outright by the development of smut pustules, the plants twisting and curving in all sorts of shapes and frequently wilting before the smut spores were mature. The third experiment was with plants 1½ feet high. Here the cornucopias were wide open and took in large quantities of the infectious fluid, which penetrated deep into the heart of the plant. After three weeks the male inflorescences appeared, but in only six plants out of 50 could any symptoms of smut be found and upon these the pustules were small and scattering. On the leaves there were wrinkled, white spots which, however, did not develop into smut pustules but subsequently became green and nearly normal in appearance. Scattered smut pustules were found on the axis at the base of the internodes in 7 cases, and the effect of the fungus was also visible on some of the upper blossoms which remained white and dried up without developing. Aside from these scattering

symptoms all of the plants remained sound, ripening normal ears. The fourth experiment, with still larger plants, gave wholly negative results. The heart of the plant proved immune, and normal ears developed. In another experiment female inflorescences were infected as soon as there was any indication of a forming ear, the Nahrlösung containing the conidia being injected into the narrow opening between the ligule and the axis. Smut pustules appeared in great numbers within 18 days but only on the parts which were actually reached by the injected fluid. Another experiment was made when the ears were in blossom. All the kernels became smutty and single ears reached the size of a child's head. In another experiment varying amounts of the lower part of the ear were protected from the fungous spray by wrapping them in blotting paper. In this case only the exposed kernels became smutty, showing again conclusively that the infection is purely local. The silk though much exposed to the conidial spray showed not the least trace of injury, having passed out of the meristematic stage. In still another experiment the kernels of the ear were sprayed with the smut conidia when they were more than ½ grown. The result was wholly negative; no smut appeared. Another experiment showed that the adventive aerial roots can also be infected if sprayed in an early stage of their growth. In short, any meristematic part of the maize plant is liable to direct infection and this is made easy by the fact, which is also Dr. Brefeld's discovery, that the corn smut fungus, unlike that of oats and sorghum, is richly provided with *aerial* conidia, which are easily carried or blown from the soil to any part of the plant. The consequent desirability of keeping the soil of corn fields free from smut spores, by removing and burning all smut pustules before they have ripened and shed, must be apparent to all. The corn smut spores seldom germinate in water, as is well known, and infection of the plant probably takes place only when the latter have an opportunity to germinate in the soil and produce the aerial conidia, this germination in the soil being greatly favored by the presence of dung. The volume contains VI, 98 pages of text and 5 lithographic plates, mostly colored.—Erwin F. Smith.

ZOOLOGY.

The Paroccipital of the Squamata and the Affinities of the Mosasauridae once more. A rejoinder to Professor E. D. Cope.—I. *The paroccipital.*—In 1870, Cope[1] designated the occipital externe, Cuvier, paroccipital, Owen with Huxley's name opisthotic, and homologized it with the squamosal of the Lacertilia and Ophidia. This opinion is held up in 1894 and in September, 1895,[2] but for the name opisthotic the name paroccipital is then used. On the other side, it is admitted by everybody else that the paroccipital, Owen (opisthotic, Huxley), which is free in the Testudines, is united with the exoccipital in the Lacertilia; the posterior portion of this bone, which is visible from behind, has been called the paroccipital process; in its anterior portion where it reaches the basioccipital it contains the posterior semicircular canals. I have stated in my last note (Am. Nat., Nov., 1895) that in young Sphenodons the paroccipital is free from the exoccipital exactly as in the Testudines and that Siebenrock has proved without question that the outer portion of the exoccipital of the Lacertilia, which lodges anteriorly the posterior semicircular canals, represents the same element. The paroccipital process of the exoccipital in Sphenodon is, of course, identical with the paroccipital process in the Lacertilia.

To this, Prof. Cope replies: "Baur asserts that the socalled parotic process [I said paroccipital process] of the exoccipital which supports the quadrate in the Squamata is the same element as that termed opisthotic by Huxley. This I deny, and believe that in this it is Baur and not myself who has fallen into error. *Siebenrock, instead of asserting this to be the case, denies it in the following language*:† 'It is not the processus paroticus of the pleuroccipital (exoccipital) which is homologous with the (paroccipital, Owen), opisthothic Huxley, but the portion anterior to the foramen nervi hypoglossi superius which protects the organ of hearing.' Siebenrock here uses the names of Owen and Huxley as refering to the same element, *but he makes the clear*

[1] Cope, E. D. On the Homologies of the Opisthotic Bone, Amer. Asso. Adv. Sc., XIX.

[2] Cope, E. D. On the Homologies of the posterior cranial arches in the Reptilia, Trans. Am. Philos. Soc., Vol. XVII, Apr. 27, 1892; also Am. Nat., May, 1892. The Osteology of the Lacertilia, Proc. Am. Philos. Soc., Vol. XXX, May 10, 1892, pp. 185–211. Amer. Nat., Sept. 1895, p. 855–856.

† Italics are mine.

distinction which is the important point, between the parotic process of the exoccipital and the element which contains the posterior semicircular canal.† What then is the element which articulates with the quadrate in the different orders of the Reptilia?"

The sentence quoted from Siebenrock is misleading. Siebenrock *does not distinguish between the parotic process of the exoccipital and the element which contains the posterior semicircular canal.* He says: *not only the parotic process but the whole portion anterior to the foramen is homologous to the paroccipital.* This whole portion, of course, contains also the parotic process. The sentence of Siebenrock translated by Cope is printed at the end of the paper in a résumé. A full account of the conditions is given on p. 209. "Die bisherige Anschauung, dass am Processus paroticus des Pleuroccipitale (exoccipitale) das Opisthoticum zu finden sei, ist daher absolut unrichtig, *sondern der ganze vordere Theil des Pleuroccipitale, welche die hintere Partie des Gehares enthält, sammt dem Processus paroticus ist als das eigentliche Paroccipitale aufzufassen.*† Vergleicht man dasselbe mit dem bei den Schildkröten zeitlebens separirten Paroccipitale, so ergiebt sich schon aus der Lage und Function die Homologie der beiden Knochen." And later: "Die gleichen Verhältnisse bestehen bei Hatteria, nur bleibt bei dersellben das Paroccipitale viel laenger vom Pleuroccipitale (exoccipitale) getrennt, als bei Lacerta."

That Prof. Cope has not studied Siebenrock's paper is also evident from the following sentence: "In the Testudinata, and according to Baur, in Sphenodon, the element which extends externally from the exoccipital to the quadrate is continuous with the opisthotic, but the semicircular canal is included in its proximal part only. Here the structure is entirely different from that which characterizes the Squamata, where the opisthotic does not extend distal of the canal and fuses early with the exoccipital." It is still more evident from the following words: "In the Squamata, where the opisthotic is restricted to the region of the canal and does not reach the quadrate, this socalled paroccipital is distinct." Cope thinks the paroccipital + otic portion of the paroccipital or opisthotic in the Testudines is not homologue to the paroccipital + otic portion of the paroccipital or opisthotic of the Squamata, and has the idea that this bone, paroccipital, Owen, opisthotic, Huxley, occipitale externe, Cuvier, consists of two elements, the outer one—the paroccipital—and the auditory portion, the opisthotic. He admits that "the direct evidence for such a primitive division of this element (occipital externe, Cuvier; paroccipital, Owen; opisthotic, Huxley)

† Italics are mine.

PLATE IV.

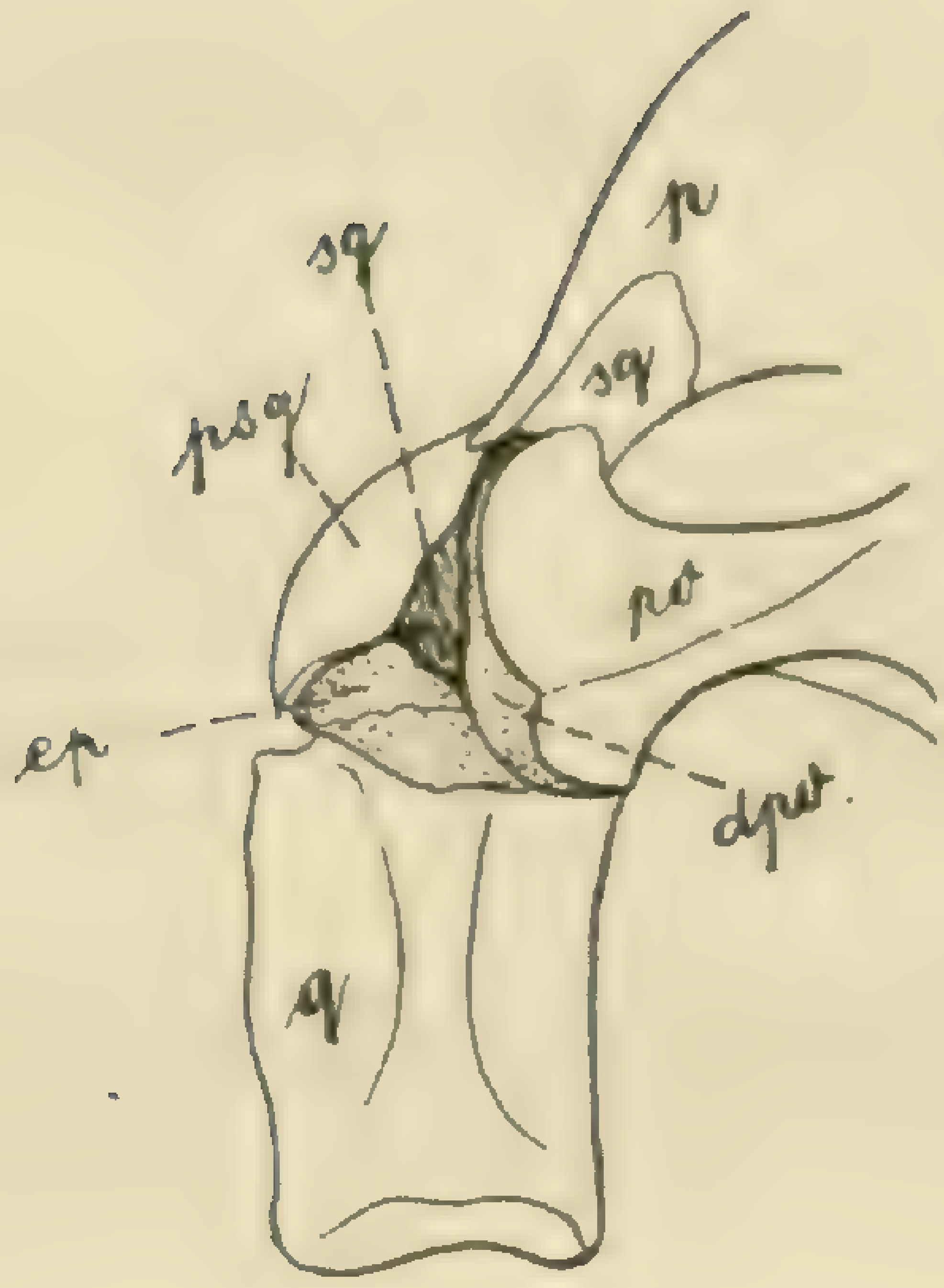


FIG. 1

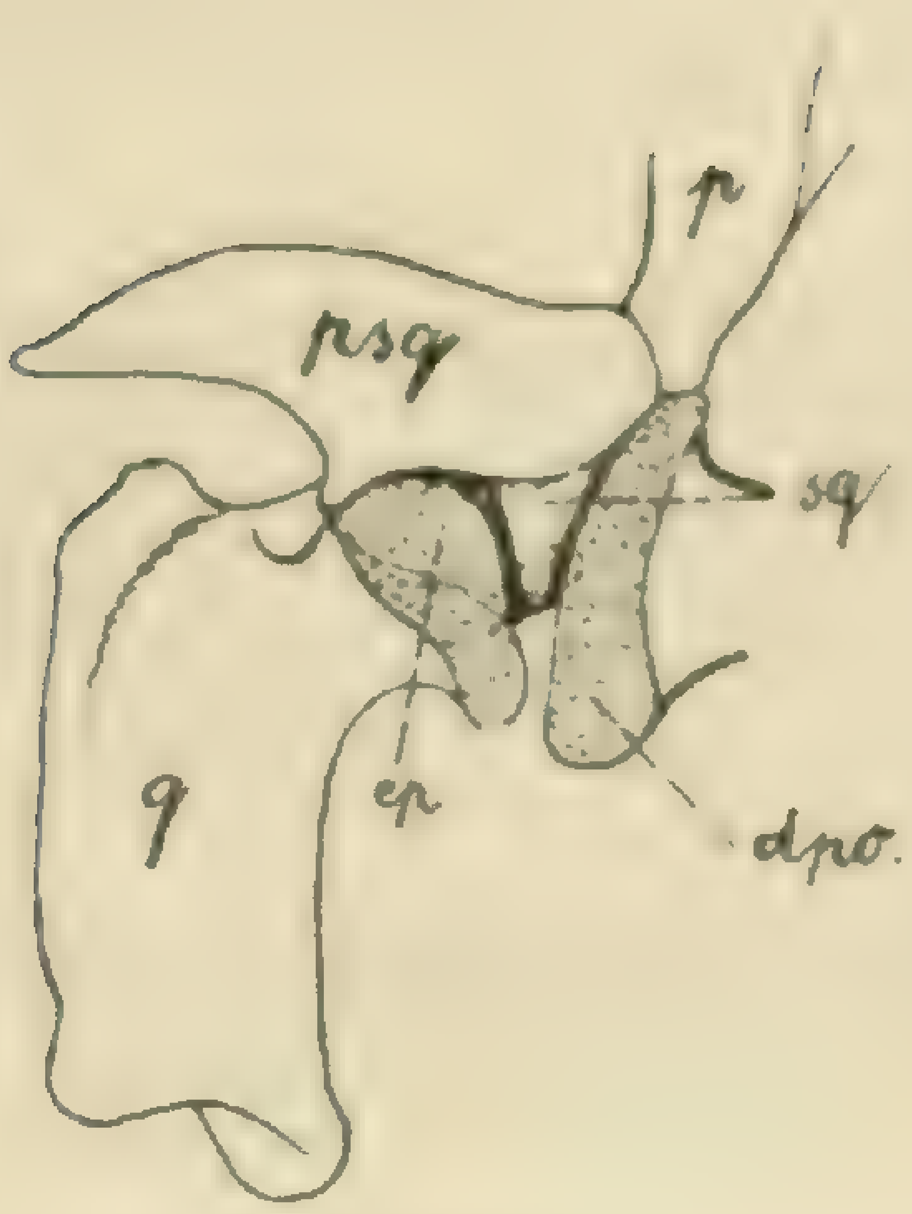


FIG. 2.

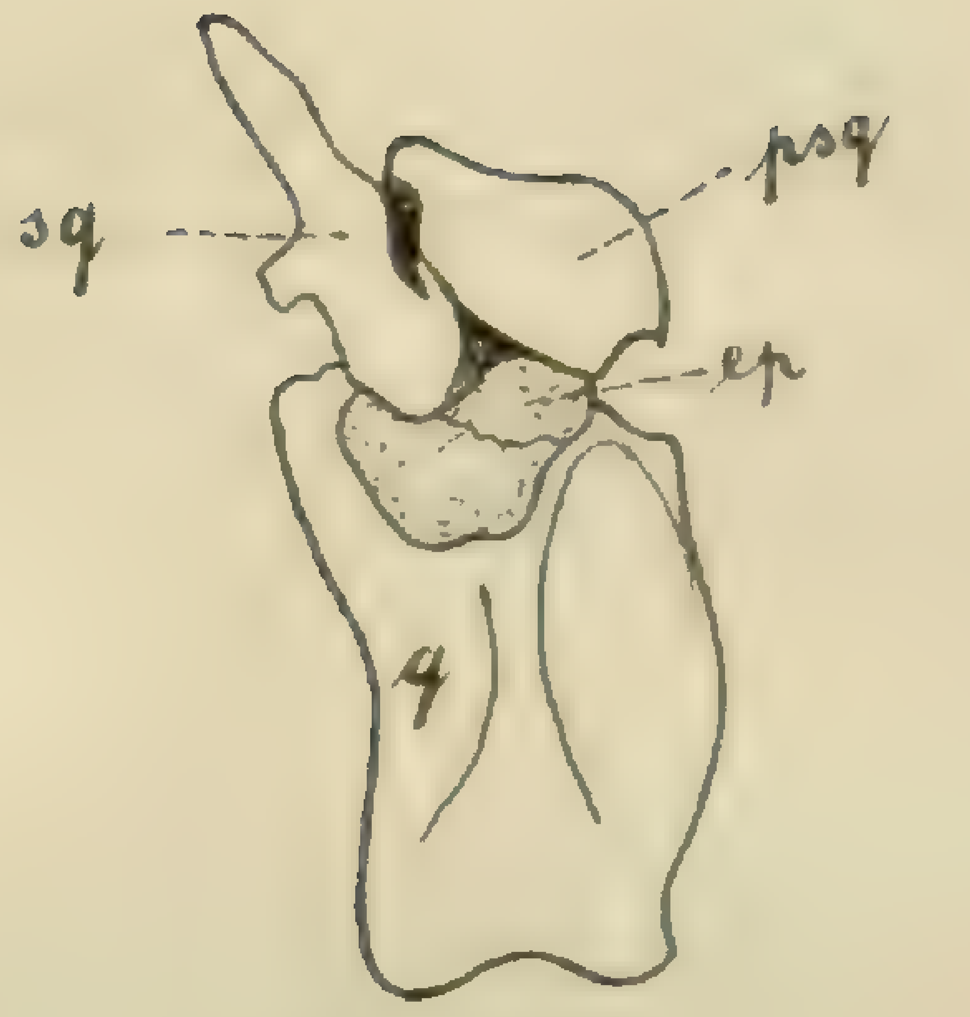


FIG. 3.

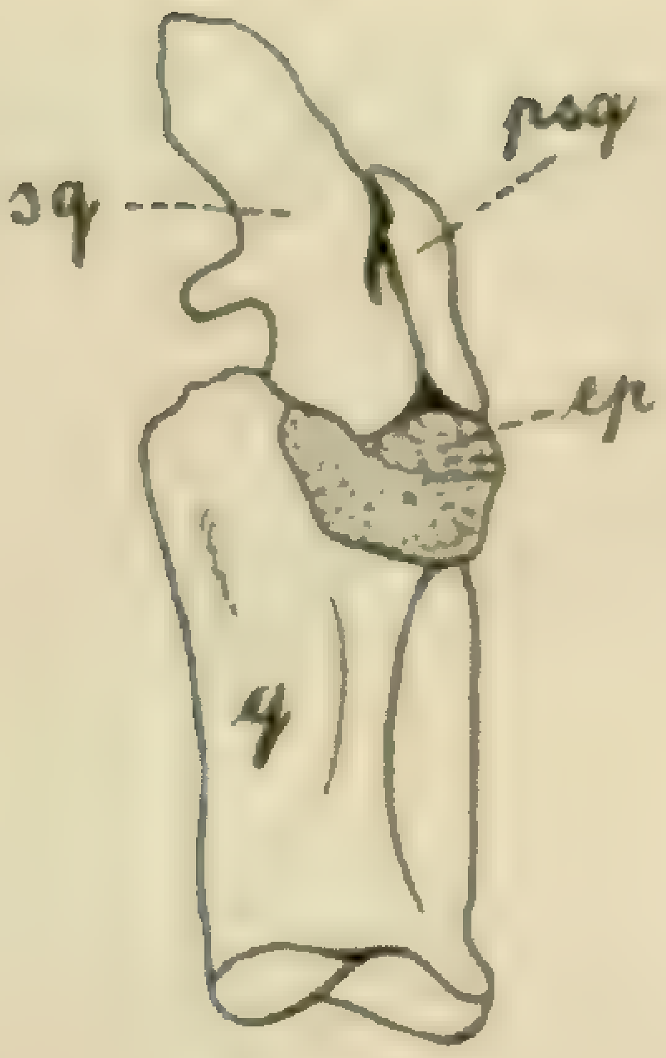


FIG. 4.

Conolophus subcristatus Gray. Quadrate and connections.

in the Testudinata has, however, yet to be produced, and I am entirely willing to give up the view above defended, should it turn out on further investigation to be untenable."

There is no further investigation necessary. The bone in question is a single element, as is shown, not only by comparative anatomy, but also by embryology. This element always is free in the Testudines; it is free in the young Sphenodon; and it is united with the exoccipital in the Squamata. There is not the slightest difficulty in this question.

One word about the squamosal. The squamosal of the Lacertilia and Ophidia is connected with the parietals and stands on the quadrate, outside of this element we have in the Lacertilia with well developed postorbital arch another element, which originally is united with the postorbital and is also connected with the squamosal and quadrate. This bone is the *prosquamosal.*| In Sphenodon the squamosal and prosquamosal are united, but in the Jurassic Saphæosaurus (Sauranodon) these two elements are free as in the Lacertilia. In the Testudines the squamosal represents the squamosal of Sphenodon, i. e., the squamosal + prosquamosal of Saphæosaurus and the Lacertilia. Prof. Cope says: "the squamosal of the Squamata is homologous to the paroccipital (opisthotic, Huxley, occipital externe, Cuvier) of the Testudines. This is impossible, since the paroccipital of the Testudines is the homologue of the paroccipital process of the Lacertilia, which in front contains, exactly as in the Testudines, the posterior semicircular canals. In the Mosasauridæ we have the same conditions as in the generalized Lacertilia. The paroccipital and exoccipital are united; connected with the quadrate we find two elements—the inner one connected by its upper branch with the parietal process; the outer one with the postorbital. These bones are, of course, homologous to the squamosal and prosquamosal of the Lacertilia.

II. *The Affinities of the Mosasauridæ.*—Cope maintains, contrary to my statement, "that in all Lacertilia the exoccipital supports the quadrate, and that in the Pythonomorpha and the Ophidia the exoccipital does not support it or generally touch it." He also maintains "that the paroccipital (squamosal, Baur) does support the quadrate in the Ophidia, while it is only in contact with a very small part of it in the Lacertilia." I have denied in my last note that in all the Lacertilia the exoccipital supports the quadrate, and I repeat it here.

I have before me disarticulated and complete skulls of Iguana, Ctenosaura, Amblyrhynchus and Conolophus. In none of these I find an articular facet on the paroccipital (exoccipital Cope), for the

quadrate. The paroccipital even does not touch the quadrate, but is connected by the anterior and upper portion of its distal process with the inner side of the squamosal; the face of the distal end of the paroccipital is entirely free from any connection and is always visible from the outside. The paroccipital process is placed behind and also above the upper face of the quadrate for these elements. In none of the genera mentioned above I find a face on the paroccipital for the quadrate, but a face for the squamosal. In the Mosasauridæ I find the same. The quadrate is supported by the squamosal and the squamosal is connected by its inner process with the anterior face of the distal end of the paroccipital; the prosquamosal takes also part in the support of the quadrate. We have, therefore, the same conditions as in the genera mentioned. The statement that the Mosasauridæ agree with the Ophidia in the relations of the quadrate, is absolutely incorrect.—G. Baur, University of Chicago.

Explanation of Figures.

Fig. 1.—*Conolophus subcristatus* Gray. Left quadrate and its relations to the squamosal, prosquamosal and paroccipital, from behind.

Fig. 2.—*Conolophus subcristatus* Gray. Left quadrate and its relations to the squamosal, prosquamosal and paroccipital, from outside and little behind.

Fig. 3.—*Conolophus subcristatus* Gray. Right quadrate and its relations to the squamosal and prosquamosal, from behind and a little inside.

Fig. 4.—*Conolophus subcristatus* Gray. Right quadrate and its relations to the squamosal and prosquamosal, from behind.

q=Quadrate.

ep=Epiphysis of quadrate.

sq=Squamosal (mastoidien, Cuvier; opisthotic, Cope, 1870; paroccipital, Cope, 1892–95).

psq=Prosquamosal (temporal, Cuvier; squamosal Cope, 1870; supratemporal, Cope, 1892–95). (Baur, G. Anat. Anz. Bd., X, p. 327.)

p=lateral process of parietals.

po=paroccipital (exoccipital, Cope).

dpo=distal end of paroccipital.

Addition.—After the manuscript of this note had been sent to the editor, I received the November number of the Annals and Magazine

of Natural History, containing a communication of Mr. G. A. Boulenger; "Remarks on the value of certain cranial characters employed by Prof. Cope for distinguishing Lizards from Snakes." Boulenger shows also that Cope's statement, in regard to the relations of the squamosum to the quadrate of the Lacertilia, is quite incorrect.

Criticism of Dr. Baur's rejoinder on the homologies of the paroccipital bone, etc.—I. *The Paroccipital Bone.*—It seems that I have not yet made clear to Dr. Baur my position as to this element in the Reptilia. The ground for it is paleontological, and when Dr. Baur considers the question from this standpoint, he will probably find some of his very positive assumptions not proveable at present.

In the first place, we agree as to the identity in the Lacertilia, Pythonomorpha and Ophidia of the element which he calls squamosal, and which I call paroccipital. Whatever be the place of this element in the Mammalian skull, it has certainly not been proven to be the squamosal, hence I object to the name which Dr. Baur uses for it, in which position I agree with various authors. It remains to be seen whether the term paroccipital, which I have hitherto used, be appropriate. I must here repeat that at no time since 1871 have I confounded it with the opisthotic of Huxley, not even in those cases (as Testudinata) where I have supposed the two elements to be fused together.

Now the characters of this paroccipital in the Pythonomorpha are such as to suggest strongly that it represents the dismembered distal part of the paroccipito-opisthotic of the Testudinata. This character I pointed out in 1870, and it deserves more attention than it has received from Dr. Baur and other authors. It cannot be seen without taking to pieces the region to which the quadrate is articulated. When this is done it is found that the paroccipital enters as a cone between the exoccipital and petrosal, and extends inwards in Mosasaurus nearly to the region of the semicircular canals. *Nothing like this is to be found in the Lacertilia.* The question now arises, what is the meaning of this structure? As the Pythonomorpha is a cretaceous type, it is evident that it is a survival of some primitive condition, and not a derivative of the condition found in the later order of Lacertilia; where the paroccipital is entirely superficial in its connections. On the contrary, the character of the Lacertilia has been more probably derived from that of the Pythonomorpha by the loss of the proximal part of the paroccipital.

In the Testudinata the paroccipito-opisthotic has not been observed, according to Baur, to consist of two elements distinct at some stage of

embryonic life. This fact does not, however, preclude the possibility that such a division may not have existed among the ancestors of the Testudinata. As this order is very old, these ancestors can only be looked for in the Permian and Triassic periods. Characters which belong to early geologic time, are frequently dropped out of the embryonic record. Now in the Permian Reptilia, some of which are the ancestors of the Testudinata, the quadrate is a short element, and is separated from the exoccipital and the opisthotic by a separate bone which has been called mastoid and mastotympanic by Owen,[3] and which I have considered as part of the "squamosal" in the absence of suture separating it from that element.[4] I think that such an element exists in the Cotylosauria. The periotic bones in Empedias[5] and Chilonyx are far removed from the elements which serve as suspensors of the quadrate bone, and are distinct from them in Chilonyx at least. Owen (l. c.) thinks that a paroccipital has been fused with the exoccipitals in Ptychosiagon (l. c.), and in a position which shows that it could not have been the opisthotic. The homologizing of one or the other of these elements with the paroccipital of the Pythonomorpha is too clearly among the possibilities to be negatived by any evidence to the contrary yet brought forward by Dr. Baur. In fact the origin of the opisthotic element as an ossification about the posterior semicircular canal, renders it a priori probable that an osseous body at a distance from that center, such as the distal part of the paroccipito-opisthotic bones in the Testudinata, was originally distinct, and for this element the name paroccipital is appropriate.

2. *The Exoccipital and Quadrate.*—Dr. Baur again denies that the exoccipital articulates with the quadrate in certain genera of Iguanidæ and gives some figures of that region in the *Conolophus subcristatus* to sustain his allegation. Unfortunately, though he seems to have taken the elements apart, as I suggested that he do, he did not put them together in their original relation when he had them drawn. I now give two drawings traced from the plate of the skull of the same species given by Dr. Steindachner.[6] As these plates represent exactly the characters which I have observed and described in allied genera, I regard them as correct. It will be observed that there is a considerable contact between the exoccipital and the quadrate. There is also con-

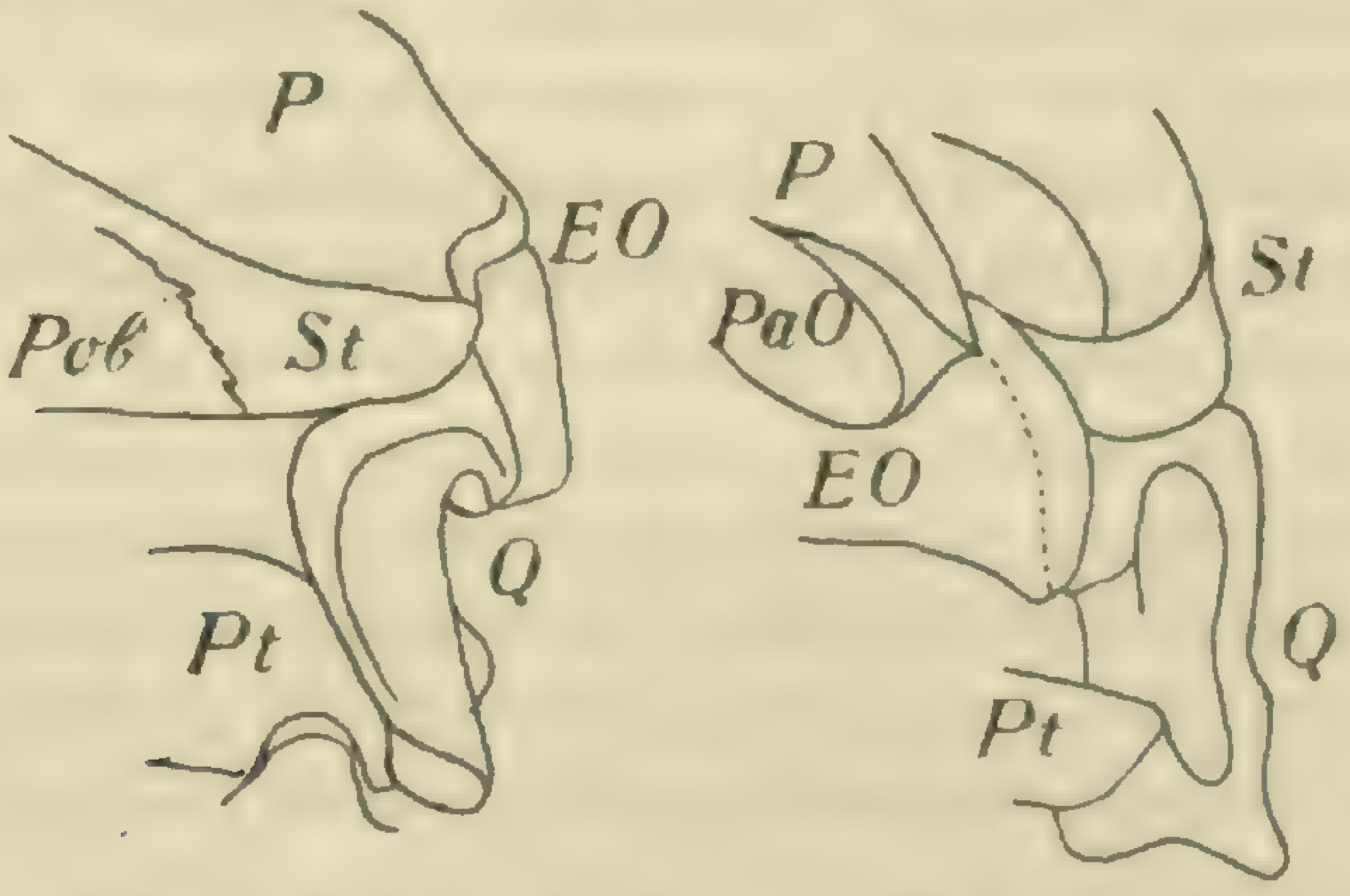
[3] Proceeds. Geolog. Soc. London, 1859, p. 50.

[4] Proceeds. Amer. Assoc. Adv. Sci., 1871, p. 207.

[5] Proceeds. Amer. Philos. Soc., 1885, p. 236; 1895, pl. VIII, fig. 4, otic region of Chilonyx.

[6] Die Schlangen u. Eidechsen der Galapagos Inseln, K. K. Zoolog. Botan. Gess. Wien, 4to. 1876; pl. V.

tact with the supratemporal, and probably with the paroccipital. *The articulation of the quadrate with the exoccipital is universal in the Iguanidæ.* I will, however, here call Dr. Baur's attention to the fact that I nowhere stated that the quadrate of the Lacertilia does not



Conolophus subcristatus Gray. From Steindachner.

touch the supratemporal or the paroccipital. I have simply asserted that the quadrate in the Lacertilia articulates with the exoccipital, and does not do so in the Ophidia. I will notice later some exceptions to the rule, of which I have since obtained information. The articulation with the supratemporal (prosquamosal, Baur) is naturally not to be mentioned in a diagnosis of the order Lacertilia, as there are numerous genera of that suborder in which that element is wanting.—E. D. Cope.

Boulanger on the Difference between Lacertilia and Ophidia; and on the Apoda.—In a recent note,[7] Dr. Boulenger criticizes my definitions of the above suborders and the Pythonomorpha as published in a late number of this journal.[8] He objects to the definition which I gave the Lacertilia, viz.: that the quadrate bone articulates with the exoccipital; and to that of the Ophidia, which asserts that this articulation does not take place. He suggests that I must have examined very few types of Lacertilia or I would not have made such a statement, and he mentions two or three other elements with which the quadrate bone also articulates. Now I must call Dr. Boulanger's attention to the fact that I nowhere state that the quadrate does not touch the other elements he has referred to, but have, on the contrary, stated that they do so touch. The doctor has apparently not read my article carefully, or has injected into his reading some-

[7] Annals and Magaz., Nat. History, XVI, 1895, p. 367.

[8] American Naturalist, 1895, p. 859.

thing which is not there. What he states in his note that bears on my definition is, that the quadrate in Chlamydosaurus does not articulate with the exoccipital, and he gives a figure to substantiate his opinion. I would have preferred to have seen a figure of this structure with the quadrate in place, but I have been able to confirm the observation by the examination of the skull of another Agamid, *Phrynocephalus olivierii.* Here the quadrate articulates with the paroccipital (supratemporal., Boul., squamosal, Baur), and is in contact with the supratemporal (squamosal, Boul.), and little or not at all with the exoccipital. How far the family of the Agamidæ generally present this structure I am unable to say, as but few skeletons of this family are at my disposal. As regards other families I have examined abundant material, as stated in my last communication (l. c., Nov. p. 1004). In review I may say that it is self evident that in general the distinction that I have drawn betweeen Lacertilia and Ophidia in this respect is valid, (1st) because the supratemporal is frequently absent, and therefore not diagnostic; (2d) because the extremity of the paroccipital is insignificant, and affords insufficient support; (3d) because the paroccipital process of the exoccipital is the only remaining element sufficient for the purpose.

Dr. Boulenger next attacks my definition of the Ophidia, alleging that the quadrate articulates with the petrosal (proötic) or with that element and the exoccipital. Here again I am supposed to have stated that the quadrate does not articulate with the (proötic) petrosal, when in fact, I did not mention that element. As to articulation with the exoccipital, I do not consider that this can be regarded as established until the embryology and paleontology are looked into. Because an element cannot be seen in an adult skull, it does not follow that it does not exist. The paroccipital is present in the Tortricidæ, and it is, so far, only an assumption to suppose that it is not represented in the allied Uropeltidæ, and in the less allied Epanodonta and Catodonta.

The decurvature of the parietals and frontals to the basicranial axis in snakes has been cited since Müller, by Huxley,[9] as peculiar to that order, and I know of no exceptions so far as regards the parietals. The optic foramina in some snakes with large eyes are confluent, as I have long been aware, and this foramen is at the expense of the inferior part of the frontals. This, however, does not produce the character of the Lacertilia, and the definition is not invalidated, as Dr. Boulenger alleges; nor is it by the decurvature of this bone and the parietal in the Amphisbænia, where they *do not reach* the sphenoid. I

[9] Anatomy of Vertebrated Animals, p. 203.

did not "admit," as alleged by Boulenger that these Lacertilia "agree with the Ophidia," as they do not. Dr. Boulenger asks "what," under these circumstances, "remains of Prof. Cope's new definition of the suborders of the Squamata?" From what has preceded it is evident, first, that they are not "new" except as to the exoccipital; and second, that they remain intact, so far as any evidence to the contrary has been produced by Dr. Boulenger, except as to the articulation of the quadrate with the exoccipital in two genera of Agamidæ. And it is not necessary to observe that very few groups so closely allied as the Lacertilia and Ophidia can be defined without exceptions.

If we now look at the definitions given by Dr. Boulenger in his volumes of catalogues of lizards and of snakes in the British Museum, the necessity of something better becomes at once apparent. The Lacertilia are thus defined: "Quadrate bone articulated to the skull; parts of the ali- and orbitosphenoid regions fibrocartilaginous; rami of the mandible united by suture; temporal region without or with only one horizontal bar. Anal cleft transverse. Copulatory organs paired. *Gunther.*"

The definition of the Ophidia (dated 1893) is as follows: "Quadrate bone articulated to the skull; brain capsule entirely osseous; rami of the mandible articulated by ligament. Anal cleft transverse. Copulatory organs present paired. *Gunther.*"

In these definitions the first and last two are identical in both. The presence or absence of a horizontal bar is not definitive, and indeed no reference to it is found in the definition of the Ophidia. The only definitions left are those derived from the mode of union of the symphysis mandibuli, and the ossification of the brain case. The former of these characters is not found in several families of Lacertilia, and the latter is the one which Dr.Boulenger has repudiated in the note which gave origin to this reply. I think my attempts at definition do, in point of precision and application, compare very favorably with those which seem to have satisfied Dr. Boulenger in the work cited. In another publication he gives the characters usually employed, which are much better.

In a recent synopsis of the species of Cæciliidæ,[10] Dr. Boulenger makes some observations on the relations of this family to the rest of the Urodela. He remarks: "If the absence of the limbs and reduction of the tail were the only characteristic of the group, I should, of course, not hesitate to unite the Cæcilians with the Urodeles; but, to say nothing of the scales, the Cæcilian skull presents features which are not shared by any of the tailed Batrachians, and the order can be

[10] Proceeds. Zool. Soc., London, 1895, p. 402.

defined by the cranial characters alone. The resemblance of the larval Ichthyophis to Amphiuma is after all superficial, and although, as I believe, the Apoda and Caudata may have evolved from a common stock, Amphiuma is certainly not the connecting form between the two as Prof. Cope would have it, for we cannot well assume the scales, lost in the Urodeles, to have reappeared in the Cæcilians."

The above discussion is interesting but troublesome, because it requires a reply. In the first place, it ought not to be necessary to remark that the presence or absence of scales in the Batrachia is not an ordinal character. On the page following that from which the above is quoted, Boulenger states that six of the sixteen genera of Apoda (Cæciliidæ) *have no scales.* Further, among the extinct Stegocephalia some genera have scales and some have none, so there is reason to suppose that scales may be secondary as well as primary. Moreover, if a a genus of salamanders should be discovered which possesses scales, no one would think of removing it from the Urodela on that account. There is no improbability in the supposition that such a genus may not be found in some of the Mesozoic formations. Second, Prof. Cope has never stated that the genus Amphiuma is the connecting form between the Apoda and Caudata. He has said that the Amphiumoidea probably are, and possibly the Amphiumidæ, but the genus Amphiuma never.[1] He has very rarely alleged that any genus is ancestral to any other genus. There can be no one genus between these two groups, for there is room for several genera. And one may agree with Dr. Boulenger that the Apoda and Caudata have had a common ancestor, and not disagree with the position that the Apoda belong to the Caudata, for there is no reason why that common ancestor may not probably have been one of the Caudata, unless there is more difference in the cranial characters of the two than has been yet pointed out.—E. D. Cope.

ENTOMOLOGY.[2]

Heterocera of the Lesser Antilles.—Reporting on a collection of Geometridæ and allied families from the islands of Grenada, St. Vincent and the Grenadines, Mr. G. F. Hampson[3] says. The Geometridæ

[1] Batrachia of N. America, 1889, pp. 34–222.

[2] Edited by Clarence M. Weed, Durham, N. H.

[3] Ann. and Mag. Nat. Hist., XVI, 329.

are represented by very few species in the Lesser Antilles compared with the large number that exist in other parts of the Neotropical Region both north and south of the isthmus; and almost all the species are identical with those found on the mainland.

The Pyralidæ are represented by a much greater diversity of species; but these, as in other parts of the world, are very wide ranging, most of the species being also found in Brazil and Venezuela, some being identical with forms found in the United States, whilst others range down to Chili; others again being spread throughout nearly the whole tropical zone; whilst, even of the species described as new, several are represented in the British Museum or other collections by specimens from continental localities.

Bot Flies of the Horse.—Prof. H. Garman publishes[3] an interesting account of the habits of oviposition of *Gastrophilus nasalis* and *G. equi*. He enumerates five species of bot flies attacking the horse in America; the adults may be distinguished by the following key:

1 (6) Discoidal cell closed by a cross vein.

2 (3) Wings marked with brown . . . . . *G. equi.*

3 (2) Wings not marked with brown.

4 (5) Anterior basal cell nearly or quite equal to the discoidal cell in length . . . . *G. nasalis.*

5 (4) Anterior basal cell markedly shorter than the discoidal cell [*G. hæmorrhoidalis.*

6 (1) Discoidal cell not closed . . . . *G. pecorum.*

Concerning the habits and life history of *G. equi*, the most abundant species, Professor Garman writes:

This fly buzzes about horses during the hot summer days, occasionally alighting on their bodies, and when an opportunity offers, placing its eggs in the hairs on the inside of the knee, on the shoulders, and sometimes even on the mane. Its mouth-parts are in a rudimentary condition, and it can not, even if it were disposed to, do any injury to horses.

It is probable that the grubs recently hatched from the eggs of this fly are taken into the mouths of horses on the lips or tongue. I am told by a gentleman who has had much experience with horses that he has on many occasions taken the eggs between the moistened palms of his hands, and in a few moments felt the young grubs wriggling about. It appears that moisture accelerates the hatching of the eggs, and it is just possible that many eggs would never hatch at all if the eggshell

[3] 7th Rept. Kentucky, Agr. Exp. Station.

was not moistened in some way. Whether this must be from the horse's tongue or lips in all cases is a question which may be considered not yet settled. Professor H. Osborn, of Iowa, is disposed to believe that the young do not hatch unless moistened by the horse's tongue; that the young grubs generally die in the eggs if left for 35 to 40 days; and that they are not commonly ready to hatch until from 10 to 12 days after the eggs are laid.

Fossil Butterflies.—Fossil butterflies are the greatest of rarities. They occur only in tertiary deposits, and out of the myriads of objects that have been exhumed from these beds in Europe and America less than twenty specimens have been found. The great body of these deposits is of course of marine origin, but at least thirty thousand specimens of insects have been recovered from those beds which are not marine. Over fifty thousand insects from the one small ancient lake of Florissant, high up in the Colorado Parks have passed through my hands, yet I have seen from them but eight butterflies. Each of these belongs to a genus distinct from the others, as is also the case with all or all but one, of the butterflies found at Radoboj, at Aix, and at Rott in the European tertiaries. With two (European) exceptions, each represents an extinct genus, and these two exceptions, Eugonia and Pontia, are genera found to-day both in Europe and America. The species, however, are all extinct.

One would hardly expect that creatures so delicate as butterflies could be preserved in a recognizable state in deposits of hardened mud and clay. Yet not only is this the case, but they are generally preserved in such fair condition that the course of the nervures and the color patterns of the wings can be determined, and even, in one case, the scales may be studied. As a rule they are so well preserved that we may feel nearly as confident concerning their affinities with those now living as if we had pinned specimens to examine; and generally speaking the older they are the better they are preserved.—*S. H. Scudder in Frail Children of the Air.*

Origin of European Butterflies.—Mr. W. H. Bath in discussing[4] the effects produced by the glacial period upon the distribution and diversity of European butterflies says: As the result of his investigations Ernest Hoffmann asserts that of the 290 species of Rhopalocera inhabiting our continent at the present time, no less than 173 were originally derived from Siberia. If this was the case, and it seems very

[4] The Entomologist, XXVIII, 247.

likely to be correct, the majority of them probably immigrated westwards of the commencement of the pleistocene periods, for they must be of great antiquity; moreover it is unreasonable to suppose that many of the species could have existed also in the south of Europe, even at the climax of the glacial period. According to the same authority only 8 species have been derived from Africa, and 39 from Asia south of Siberia. These must have immigrated into the south European province of the palearctic region after the termination of the glacial period as they belong to genera and types of tropical distribution. At the present day they occur in those countries bordering on the Mediterranean Sea.

The glacial species of butterflies—that is the most ancient forms, designated by Weismann "the original stirps"—are in many cases distinguished by their melanic and melanochroic tendencies. We thus find the forms inhabiting the more northern localities and the higher elevations on the mountains often of a darker hue, while their representatives in more southern latitudes and less elevated altitudes exhibit a brighter coloration.

North American Aphelininæ.—As the first of a technical series of bulletins to be issued by the Division of Entomology of the U. S. Department of Agriculture, Mr. L. O. Howard publishes a Revision of the Aphelininæ of North America. Regarding the biology of the group Mr. Howard writes: The insects of this subfamily are all, so far as we know, parasitic either upon the Coccidæ, Aleyrodidæ, or Aphididæ. They are evidently many brooded, and issue from their hosts indifferently throughout the warmer months of the year, and through the winter on the insectary. With the Aleyrodidæ, Aphididæ, and the Diaspinæ among the Coccidæ, but one specimen apparently issues from a single host. Sufficient observations have not been made upon the early stages of the Aphelininæ. Their larvæ feed both upon the body of the scale insect and upon the eggs. They attack both sexes of the host, issuing when full-grown through circular holes, cut through the body walls, and, in the case of the Diaspinæ through the scale. With the scale insects of the genus Pulvinaria, the aphelinine larvæ live within the body of the female and not in the waxy egg mass which she secretes.

News.—A List of Night-flying Moths from Kentucky, is published by Prof. H. Garman in the 7th Report of the Experiment Station of that State.

An extended account of the life-history of *Phryganidia californica* Packard is published by Messrs. V. L. Kellogg and T. J. Jack in the Proceedings California Academy of Sciences (Ser. 2, V, 562–570.)

Prof. J. B. Smith issues as Bulletin 111 of the New Jersey Experiment Station an account of experiments with "Raupenlime" and "Dendrolene," substances useful for applying to tree trunks to keep out borers.

PSYCHOLOGY.[1]

American Psychological Association.—The American Psychological Association held its annual meeting this year at the University of Pennsylvania, in connection with the meetings of the scientific societies affiliated with the American Society of Naturalists. Hitherto the Psychological Association has met independently, but the feeling has been growing that the close relation between the more recent forms of psychology and the biological sciences made it eminently suitable and desirable that their representatives should be brought together. The success which has attended this first step makes it probable that the policy will be continued in future.

No official outline of the proceedings of the Psychological Associaciation is at hand, and any account written from memory will be more or less defective. Consequently the present writer must beg indulgence from those whose words he endeavors to report if he has, in any case, misrepresented them. On the whole, however, he believes he is giving a fair outline of the more important points.

At the first session, on Friday, Dec. 27th, the opening paper, on "Physiology and Psychology," was read by Prof. George S. Fullerton of the University of Pennsylvania. Two years ago, at the New York meeting of the Association, Prof. Fullerton outlined the relation in which psychology as a natural science stands to metaphysic, and concluded that psychology should adopt, as far as possible, the methods and assumptions of the other natural sciences, and should relegate the task of criticising those assumptions to a distiuct science—that of metaphysic. The paper read this year was a continuation of the same general line of thought in the investigation of the relations of psychology and physiology. Taking Foster's "Physiology" as a standard, we find, said Prof. Fullerton, that the author is absolutely unable to give any

[1] This department is edited by Dr. Wm. Romaine Newbold, University of Pennsylvania.

account of the functioning of the higher nervous centres without having recourse to sensations, ideas, volitions—in a word, without entering the field that properly belongs to psychology. While it may be not only right, but also necessary, for the physiologist to do this, we must not close our eyes to the fact that the mere fact of its necessity proves the imperfect condition of physiology, and tends to obscure the line dividing physiology from psychology. Prof. Fullerton claimed that the methods employed by the two sciences are distinct, and that it is important to the advancement of knowledge to recognize this distinction.

Dr. Livingston Farrand, of Columbia, submitted a scheme of physical and mental tests which will be used with the students of Columbia to determine, as far as can be done by direct experiment, their capacities in both respects at various stages of their college life. After some discussion, a motion was passed that the President be requested to appoint a committee of five to report upon the advisability of the universities represented taking concerted action in the adoption of some similar scheme.

Dr. Arthur MacDonald, of Washington, D. C., read a paper on "Some Psycho-Neural Data." He reported experiments somewhat similar to those of Dr. Farrand, made upon certain groups in the community, and apparently showing that between definite classes definite physical and mental differences are experimentally discoverable.

Prof. Lightner Witmer, of the University of Pennsylvania, introduced one of his graduate students, Mr. Oliver Cornman, who reported the results of "An Experimental Investigations of the Processes of Ideation." Mr. Cornman's method was that of giving a large number of individuals, usually children, a definite suggestion and requiring them to write for a definite period of time—usually 15 minutes—all the thoughts directly or indirectly suggested by it; he had found that in most of his subjects tne idea trains were, for a short time, largely controlled by the concomitant suggestions of the time and place, and consequently the earlier terms of each series showed a marked similarity. This soon disappeared, and the further development of the idea trains seemed dependent upon the character and previous experience of the individual. We have, therefore, in this, a convenient method of "tapping," as it were, the ideational content of the individual. Mr. Cornman pointed out further, that, to get results at all comparable with one another in the case of different bodies of subjects, the original suggestions must be given in identically the same words without explanations or further suggestions on the part of the experimenter, and, to secure this end, should always be written.

At the afternoon session on Friday, Prof. J. McK. Cattell, of Columbia, read his President's Address. It was, on the whole, a defense of that experimental method of which he is the leading representative in this country, and was, therefore, in a way, a reply to the rather unfavorable estimate of the method and its results which had been expressed by Prof. James of Harvard in his President's Address of the preceding year. The burden of Prof. Cattell's argument was found in the statement, that every science is either genetic or quantitative in its method; that those sciences which have been predominently quantitative will undoubtedly, in time, be formulated in genetic terms, that, conversely, into the genetic sciences also, such as biology and psychology, the quantitative method will ultimately be introduced. This is the aim of experimental psychology in the narrower sense. While expressing the strongest conviction of the importance of this experimental method to the science of psychology, Prof. Cattell displayed such moderation in his estimate of the results thus far achieved by it, and such sympathetic insight into the aims and relative values of other methods, that his address was received with the warmest applause by all, and no one could be found to pass a criticism upon it.

Prof. Chas. A. Strong, of the University of Chicago, read a paper on "Consciousness and Time," of which, on account of its exceedingly abstract character, I could not venture to give an analysis from memory.

The morning of Saturday, December 28th, was occupied by a discussion on "Consciousness and Evolution."

Prof. William James, of Harvard, opened the discussion by outlining the general features of the problem at issue: First, whether consciousness is coextensive with the universe or originated in time; second, whether consciousness is an active force capable of controlling brain movement, or whether it is a mere epiphenomenon, produced by the brain but not capable of affecting the brain; third, whether consciousness has been a factor in the production of adaptation.

Prof. Cope, of the University of Pennsylvania, who had been especially requested to take the leading part in the discussion, attacked the question from the point of view of the paleontologist. He held that natural selection is not sufficient to account for adaptation, that the adaptation of the individual organ is the result of use, and that the effects of use can be inherited. In supporting this position he gave many illustrations, based upon his personal observation. He held further that organic evolution involved combinations and recombinations of matter which not only never could have been produced by the opera-

tion of known physical and chemical forces, but were of a character precisely the opposite of their known effects. To account for this, he thought we must assume in organic matter the existence of an activity distinct from all the other activities of nature. Progressive evolution is the chief outcome of this activity, and therefore he had proposed to term it an anagenetic, or upbuilding activity, as opposed to the katagenetic or destructive activities of physics and chemistry. This anagenetic activity Prof. Cope was inclined to believe due to the presence of sensation, and therefore maintained that consciousness is an active factor in the individual and in evolution.

Prof. Cope was followed by Prof. J. Mark Baldwin, of Princeton, who commented upon several points of Prof. Cope's argument, drawing special attention to the fact that recent investigation into the effect on young children of their surroundings makes it more easy to account for adaptation without reference to inheritance of acquired aptitudes. He also deplored the sharp antithesis between the doctrine of consciousness as a cause and as a epiphenomenon, holding that both views found their reconciliation in monism.

Prof. C. Sedgwick Minot, of Harvard, attacked the neo-Lamarckian doctrine from the neo-Darwinian point of view, supporting his position by evidence drawn from his own work in embryology. He suggested, as a speculation, that consciousness, although not itself a force, might be conceived to possess the property of selecting out of the brain forces that one which it is control conduct.

Prof. G. S. Ladd, of Yale, welcomed Prof. Cope's address as an important contribution from the purely scientific point of view to the support of doctrines held by himself in common with many other metaphysicians, and made a plea for the recognition of the metaphysician on the part of scientists as a coworker in the field of knowledge.

Prof. Fullerton, of the University, called attention to our actual ignorance on all these points, and expressed the opinion that fundamental differences exist which cannot be glossed over by such vague doctrines as that of monism.

Other speakers were: Prof. J. H. Hyslop, of Columbia; Dr. D. S. Miller, of Bryn Mawr, and Dr. Wesley Mills, of McGill University, Montreal.

Prof. Cope then concluded the discussion by adducing a series of arguments in favor of the inheritance of acquired attributes, any one of which, he held, would be sufficient to set the matter at rest.

At the afternoon session, Prof. G. T. W. Patrick, of the University of Iowa, reported an experiment on the effects of loss of sleep. A patient

had been kept awake for 90 consecutive hours, during which time careful experimental tests were made of his physical and mental condition, and the results were reported in detail. Among the more interesting of these results were, continuous increase in weight, relatively slight loss of muscular strength, the production of visual hallucinations, and the sudden disappearance of all symptoms after only 10½ hours of sleep—about 25 per cent. of that which had been lost.

Prof. Wesley Mills, of McGill University, Montreal, announced his intention of contributing at the next meeting of the Association further researches on the psychic development of young animals and its physical correlations.

Prof. Lightner Witmer, of the University of Pennsylvania, read a paper on "Variations in the Patellar Reflex as an Aid in Mental Analysis. Dr. Witmer described the apparatus and the method used to determine, 1st, The extent of the normal jerk ; 2d, the increment due to the synergic activity of the cortical processes concerned in sensation, thoughts, etc. His results he regarded as tentative only ; they appeared, however, to show (1) that sensation or thought processes which did not directly tend to produce movement had little effect upon the knee jerk ; (2) that all processes which tended to produce muscular contraction in any part of the body tended to increase the knee jerk ; (3) that this increase was quite as marked in the case of the *thought* of a movement as in that of the movement itself.

Prof. James H. Hyslop, of Columbia, reported a series of experiments on hallucinations induced by a crystal. He did not attempt to give any explanation of the phenomena, but pointed out that in two cases the phantasms possibly indicated some unknown method of acquiring information.

Prof. W. R. Newbold narrated informally three cases vaguely described as "Dream Reasoning," which had occurred in the experience of two of his colleagues. Dr. W. A. Lamberton, Professor of Greek in the University of Pennsylvania, when a young man, after giving up as insoluble a problem in descriptive geometry upon which he had been working for weeks by the analytical method, awoke one morning several days later to find an hallucinatory figure projected upon a blackboard in his room with all the lines necessary to a geometrical solution of the problem clearly drawn. He has never had any other visual hallucination. Dr. H. V. Hilprecht, Professor of Assyriology in the University of Pennsylvania, some years ago dreamed an interpretation of the name Nebuchadnezzar which has since been universally adopted. At a later period he dreamed that an Assyrian priest

gave him information about some inscribed fragments that had puzzled him which was afterwards confirmed in all points now capable of confirmation. Dr. Newbold offered a psychological explanation of these curious cases.

Prof. G. S. Fullerton, of the University of Pennsylvania, was elected President, and Dr. Livingston Farrand, of Columbia, Secretary, for the ensuing year.

Among the members present, besides those already mentioned, were Mr. Henry Rutgers Marshall, of New York; Prof. N. S. Gardiner, of Smith College; Dr. H. C. Warren, of Princeton; Prof. E. S. Sanford, of Clarke University; Prof. E. H. Griffen, of Johns Hopkins; Prof. J. C. Creighton, of Cornell; Prof. James Seth, of Brown, and Dr. Warner Fite, of Williams' College.—W. R. N.

The Cat's Funeral.—Every one has observed instances of affection between those proverbially hostile animals, the dog and the cat, but a case cited by l'Eleveur merits especial attention. A dog and a cat belonging to the same master were the best friends in the world, and spent their time in frolicking together. One day, while playing as usual, the cat died suddenly, falling at the dog's feet. The latter, at first, did not realize what had happened, but continued his play, pulling, pushing and caressing his companion, but with evident astonishment at her inertness. After some time he appeared to understand the situation, and his grief found vent in prolonged howls. Presently he was seized with the idea of burying the cat. He pulled her into the garden, where he soon dug a hole with his paws, and put in it the body of his former companion. He then refilled the hole with dirt, and, stretching himself out on the grave, resumed his mournful howling. The idea of burying the dead cat was extraordinary. Whence came the thought? Could it be imitation, or, which is a better explanation, did the dog have a vague idea of concealing the event which might possibly be imputed to him. But then it would seem unreasonable for him to call attention to the fact, by installing himself on the grave and howling. However, even human criminals are sometimes equally inconsistent. It is difficult to form an exact idea of what gave rise to the dog's conduct in this case. (Revue Scientific Juillet, 1895).—E. D. C.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

American Academy of Arts and Sciences.—The 11th of December.—The following papers were read: On the temperature of the crust of the earth at great depths. By Messrs. Alexander Agassiz and P. C. F. West. Palestine in the fifteenth century B. C. according to recent discoveries. By Professor Crawford H. Troy.

Boston Society of Natural History.—December 4th.—The following paper was read: Mr. L. S. Griswold, "The San Francisco Mountains and the Grand Canyon."

December 18th.—The following paper was read. Prof. G. Frederick Wright, "The present status of glacial man in America." The subject of Professor Wright's paper was discussed by Prof. F. W. Putman, Prof. H. W. Haynes, and others.

January 1st, 1896.—The following papers were read: Mr. A. W. Grabau, "Lake Bouvé, a glacial lake in the Boston Basin;" Prof. W. O. Crosby, "Glacial lakes in the valleys of the Neponset and Charles Rivers; and the Post-tertiary history of the Nashua Valley.—SAMUEL HENSHAW, *Secretary.*

January 15th.—The following paper was read: Mr. William Brewster, Notes on the Natural History of Trinidad. Stereopticon views were shown.—SAMUEL HENSHAW, *Secretary.*

New York Academy of Sciences, Section of Biology.—December 9, 1895.—The following papers were presented: Prof. C. L. Bristol, "The Classification of Nephelis in the United States." The study of abundant material, collected from Maine to South Dakota, has shown that the color characters cannot be depended upon for specific determination. An examination of the metameral relations of this leech indicate that not more than a single species occurs in this country. Prof. F. H. Osborn, "Titanotheres of the American Museum of Natural History." The complete skeleton of *Titanotherium robustum* is remarkable in possessing but twenty dorso-lumbar vertebræ, a number identical with that typical of the Artiodactyla, but entirely unique among Perissodactyla. It is now appears probable that the development of horns in the Titanotheres became a purely sexual character, and that the genera Titanops, Marsh and Brontops, Marsh, are founded respectively upon male and female individuals of *Titanotherium robustum.* Dr. J. L. Wortman, "The expedition of 1895 of the Amer-

ican Museum of Natural History." The Expedition passed into the Uinta beds of N. E. Utah, then between the Eastern escarpment of the Uinta range and the Green River into the Washakie Beds of S. W. Wyoming, the most important result geologically being that the Brown Park deposit is found to be of much later age then the Uinta.—BASHFORD DEAN, *Recording Secretary.*

American Philosophical Society.—The following communications were read: "The Use of Photography for the Detection of Differences in Chemical Composition, in Age, and in Fluidity of Inks," Prof. S. P. Sharples. "Some Observations on the Forgery of a Mark," and "Detection of a Forgery in the Fraudulent Use of a Signature Stamp," Dr. Persifor Frazer.

Academy of Natural Sciences.—Philadelphia, December 31st.—The following officers were elected: President, Samuel G. Dixon, M. D.; Vice-Presidents, Thomas Meehan, Rev. Henry C. McCook, D. D.; Recording Secretary, Edward J. Nolan, M. D.; Corresponding Secretary, Benjamin Sharp, M. D.; Treasurer, George Vaux, Jr.; Librarian, Edward J. Nolan, M. D.; Curators, Henry A. Pilsbry, Henry C. Chapman, M. D., Arthur Erwin Brown, Samuel G. Dixon, M. D.; Councillors to Serve Three Years, Uselma C. Smith, William Sellers, Charles E. Smith, John Cadwalader; Finance Committee, Charles Morris, Chas. E. Smith, Uselma C. Smith, William Sellers, Charles P. Perot; Council, Isaac J. Wistar.

The American Morphological Society held its annual meeting at the University of Pennsylvania, Dec. 26, 27, and 28, 1895. The stated business of the first session was the Report of the Committee of Affiliation with the American Society of Naturalists. After considering this report the Society voted against affiliation. The following were elected to membership: C. J. Herrick, Denison University, Granville, Ohio; E. G. Conklin, University of Pennsylvania, Philadelphia, Pa.; F. R. Lillie, University of Michigan, Ann Arbor, Mich.; F. C. Kenyon, Clark University, Worcester, Mass.; T. H. Montgomery, Jr., West Chester, Penna.; J. L. Kellogg, Olivet College, Olivet, Mich.; J. I. Peck, Williams College, Williamstown, Mass.; and A. D. Meade, Providence, R. I.

At the second session, December 27, the following papers were read and discussed: "Panplasm," by Prof. C. S. Minot; "The History of the Centrosome in Thalassema," by Mr. B. B. Griffin; "The Centrosome in its Relation to Fixing and Staining Agents," by Prof. E. B. Wilson; The Production of Artificial Archoplasmic Centers," by Prof.

T. H. Morgan; "Cell Size and Body Size," by Prof. E. G. Conklin; "The Development of Isolated Bastomeres of the Egg of Amphioxus," by Prof. T. H. Morgan; and "On the Smallest Part of Stentor Capable of Regeneration," by F. R. Lillie (read by the Secretary). The following officers were elected for the ensuing year: President, Prof E. L. Mark, Harvard University; Vice-President, Prof. H. F. Osborn, Columbia College; Secretary and Treasurer, Dr. G. H. Parker, Harvard University. Members of the Executive Committee elected from the Society at large, Prof. E. G. Conklin, University of Pennsylvania, and Prof. W. Patten, Dartmouth College.

At the third session, December 28, the following papers were read and discussed: "Gastrulation of Teleosts," by Dr. Bashford Dean; "Pigment Changes in the Eye of Palæmonetes," by Dr. G. H. Parker" "Reaction of Metridium to Food and other Substances," by Dr. G. H. Parker; "Some Points in the Anatomy of Anoplocephaline Cestodes," by Dr. C. W. Stiles; and "Development of Cassiopea from Buds," by Dr. R. P. Bigelow. After passing resolutions of thanks to the University of Pennsylvania, the American Philosophical, Society, and the Philadelphia Local Committee, the Society adjourned *sine die.*

The American Society of Naturalists,—Met in the Hall of Department of Arts and Sciences of the University of Pennsylvania, on Thursday December 26th and Friday, December 27th, 1895. *Thursday, Dec. 26th, 2 P. M.* I. Reports of Committees. II. Special Reports. III. Recommendation of new members. IV. Address by the President, E. D. Cope. "The Formulation of the Natural Sciences." V. Special Papers, Prof. B. Wilder on the teaching of Comparative Anatomy. 8 *P. M.* Illustrated Lecture at the Hall of the Academy of Natural Sciences, by Professor W. B. Scott, of Princeton University, on "The American Tertiary Lakes and their Mammalian Faunas." 9 *P. M.* Reception to all the Societies given by Professor Horace Jayne, at his house on the S. E. corner of 19th and Chestnut Streets. *Friday, December 27th, 9 A. M.* The following new members were elected: Professor C. L. Bristol, Dr. F. C. Kenyon, Dr. W. E. Rotzell, L. O. Howard, Professor John Dewey, G. H. Girtz, Dr. A. D. Mead, Professor G. S. Fullerton, Professor J. McK. Cattell, Professor G. T. Ladd, Reid Hunt, Professor William James, Dr. F. Baker, Dr. G. E. Stone, Professor J. M. Baldwin, Dr. T. S. Palmer, George Lefever,

The following officers were elected for the ensuing year: President, Prof. Wm. B. Scott, of Princeton College; Vice-Presidents, Prof. Wm. G. Farlow, of Harvard; Prof. C. O. Whitman, of Chicago University; Dr. Theodore Gill, of the Smithsonian Institution; Secretary, Dr. H.

C. Bumpus, of Brown University; Treasurer, Prof. John B. Smith, of New Brunswick, N. J.; Executive Committee, Prof. Horace Jayne, of Philadelphia, and Prof. Wm. F. Ganong, of Smith College, Mass.

The following committees were apppointed: On Vivisection; Drs. Patton, Sedgwick and Stiles. On the American table at the Naples Zoological Station; Drs. Conn and Stiles. On Antarctic exploration; Professors Heilprin, Osborn and Goodale. The Society elected Prof. E. D. Cope as its representative on the committee to consult with the American member of the committee of the International Congress of Zoologists on Nomenclature. 10 *A. M.* Discussion. *Subject:* The Origin and Relations of the Floras and Faunas of the Antarctic and Adjacent Regions. Geology. Prof. Angelo Heilprin, Philadelphia Academy Natural Sciences. Paleontology. Prof. W. B. Scott, Princeton University. 2 *P. M.* Continuation of the Discussion. Botany. Prof. N. L. Britton, Columbia College. Zoology. Vertebrata, Dr. Theo. Gill, Smithsonian Institution. 7.30 *P. M.* Annual Dinner of the Affiliated Societies at the Lafayette Hotel, north-west corner of Broad and Sansom streets.

Association of American Anatomists.—This body met in Philadelphia, on Dec. 27th and 28th, at the University of Pennsylvania.—*Friday Morning, December 27th*, 8.30 o'clock.—Meeting of Executive Committee. 9.30 o'clock.—Opening of the session by the President. Report of Secretary and Treasurer. Report of Executive Committee. Report of Delegate to Congress of American Physicians and Surgeons. Report of Committee on Anatomical Nomenclature. Report of Committee on Anatomical Material. Report of Committee on Circular concerning Anatomical Peculiarities of the Negro. Report of Dr. Allen, of the Smithsonian Committee on the Table at Naples. Election of members. Other new business. Reading of Papers and Discussions.—1. "Myology of the Extremities of Lemur bruneus" illustrated by drawings and casts of muscles. Dr. George S. Huntington, N. Y. City; 2. "History of the Ciliary Muscle," Dr. Frank Baker, Washington, D. C.; 3. "Absence of Fibrous Pericardium of left side." Illustrated by specimen, Dr. Addinell Hewson, Philadelphia, Pa. "The Descriptive Anatomy of the Human Heart," Dr. Wm. Keiller, Galveston, Texas. *Friday Afternoon*, 2.30 oclock.—Miscellaneous business. Reading of Papers and Discussions.—5. "Nomenclature of Nerve Cells," Dr. Frank Baker, Washington, D. C.; 6. "The Cerebral Fissures of two Philosophers." Illustrated by specimens and photographs, Dr. B. G. Wilder, Ithaca, N. Y.; 7. "The Human Paroccipital Fissure. Should it be recognized and so Designated." Illustrated by specimens and photographs, Dr.

Wilder; 8. "Practical Histology for large classes" Dr. Chas. S. Minot, Boston, Mass. In the evening a subscription dinner was given by the members of the affiliated societies at the Hotel Lafayette. *Saturday Morning, December 28th.*—Miscellaneous business of minor importance was transacted and these officers elected: Dr. Frank Baker, of Washington, D. C., President; Dr. B. G. Wilder, of Ithaca, N. Y., First Vice-President; Dr. F. J. Shepherd, of Montreal, Canada, Second Vice-President; Dr. D. S. Lamb, of Washington, D. C., Secretary and Treasurer; Delegate to Congress of American Physicians and Surgeons, Dr. Addinnell Hewson, Philadelphia; Alternate, Dr. D. K. Shute, of Washington, D. C. Reading of Papers and Discussions.—9. "Some novel methods of description of the human skull" Dr. Harrison Allen, Philadelphia, Pa.; 10. "Type forms and nomenclature of mammalian teeth." Illustrated by models and diagrams, Prof. Henry F. Osborn, New York City; 11. "The work of the German Anatomical Society in Nomenclature," Dr. Charles Heitmann, New York City. *Sunday Afternoon,* 2.30 o'clock.—Miscellaneous business. Reading of Papers and Discussions; 12. "*Fossa capitis femoris* with observations on the trochanteric fossa." Illustrated by specimens, Dr. F. J. Brockway, New York City; 13. "Note on the appearance of a unilateral tuberosity in place of the trochanteric fossa." Illustrated by specimen, Dr. D. S. Lamb, Washington, D. C.

The American Physiological Society.—The eighth Annual Meeting of the American Physiological Society was held in Philadelphia on December 27th and 28th, 1895. The meeting was preceded by the usual smoke talk upon the evening of December 26th. Three of the four formal sessions of the Society were held at the University of Pennsylvania, the fourth at the Jefferson Medical College, The following communications were presented and discussed:

R. H. Chittenden, The mucin of the white fibrous connective tissue; A. R. Cushny, The distribution of iron in the Invertebrates; J. J. Abel, A preliminary account of the chemical properties of the pigment of the negro's skin (with W. S. Davis); T. B. Aldrich, On the Chemical and physiological properties of the fluid secreted by the anal glands of Mephitis mephitica; G. Lusk, Phloridzin diabetes and the maximum of sugar from proteid; W. T. Porter, Further researches on the coronary arteries; G. N. Stewart, Note on the quantity of blood in the lesser circulation; C. F. Hodge, Histological characters of lymph as distinguished from protoplasm; C. F. Hodge (for J. R. Slonaker), Demonstration of the comparative anatomy of the fovea centralis; G. C. Huber, The ending of the chorda tympani in the sublingual and the

submaxillary glands (with demonstrations); G. W. Fitz, A working model of the eye; J. G. Curtis, A method of recording muscle curves; G. N. Stewart, Demonstration: Measurement of the circulation time of the retina; T. W. Mills, Cortical cerebral localization in certain animals; W. T. Porter, A new method for the study of the intracardiac pressure curve; S. J. Meltzer, On the mode of absorption from the peritoneal cavity in rabbits, (with I. Adler); S. J. Meltzer, On the incorrectness of the often quoted experiments of Starling and Tubby with reference to the mode of absorption from the peritoneal cavity in dogs; F. S. Locke, Of the action of ether on contracture and of positive kathodic polarisation of voluntary muscle; H. G. Beyer, On the influence of exercise on growth; W. H. Howell (for Messrs. Conant and Clark), The existence of a separate inhibitory and accelerator nerve to the crab's heart; Fr. Pfaff, On Toxicodendral and on the socalled toxicodendric acid; H. C. Chapman, Methods of teaching physiology.

The following persons were elected to membership in the Society:

J. G. Adams, Professor of Pathology, McGill University; T. B. Aldrich, Instructor in Physiological Chemistry, Johns Hopkins University; J. M. K. Cattell, Professor of Experimental Psychology, Columbia College; G. P. Clark, Professor of Physiology, Syracuse University; R. H. Cunningham, Assistant Demonstrator of Physiology, Columbia College; G. W. Fitz, Assistant Professor of Physiology and Hygiene, Harvard University; T. Hough, Assistant Professor of Physiology, Massachusetts Institute of Technology; R. Hunt, Fellow in Physiology, Johns Hopkins University; F. S. Locke, Instructor in Physiology, Harvard Medical School.

Professors C. S. Minot and C. F. Hodge were appointed to express to Professor Langley the opinion of the Society that it is highly desirable that the table of the Smithsonian Institution at the zoological station of Naples be continued. Mr. W. B. Saunders entertained the members of the Society at luncheon at the Art Club. The courtesies that were extended to the affiliated societies by the University of Pennsylvania and the Philadelphia Local Committee were also enjoyed. Officers for the year 1895–96 were elected as follows:

Members of the Council: H. P. Bowditch, R. H. Chittenden, W. H. Howell, F. S. Lee, J. W. Warren; President, R. H. Chittenden; Secretary and Treasurer, F. S. Lee.

The President and the Secretary were appointed respectively delegate and alternate to the Congress of American Physicians and Surgeons of 1897.—FREDERIC S. LEE, *Secretary.*

The Geological Society of America held its eighth Annual Meeting, and the fifteenth meeting of the Society in the Geological Museum of the University of Pennsylvania, December 26th to 28th. The number of Fellows in attendance was sixty. The first session was convened at 2 o'clock on Thursday afternoon with President N. S. Shaler in the chair. The report of the Council, consisting of the detailed reports of the officers for the year 1895, was submitted in print. This report showed a properous condition of the Society; following are some of the items: membership 226, libraries subscribing for the bulletin 59, receipts during the year from the sale of the bulletin $461.50, number of exchanges 85. The library is deposited with the Case Library at Cleveland. Besides printing six volumes of the bulletin, $3000 has been invested as a publication fund.

Announcement was made of the election by transmitted ballots of officers for 1896 as follows:

President, Joseph LeConte; First Vice-President, C. H. Hitchcock; Second Vice-President, Edward Orton; Secretary H. L. Fairchild; Treasurer, I. C. White; Editor, J. Stanley Brown; Councillors, B. K. Emerson, J. M. Safford.

The following Fellows were declared elected: Harry F. Bain, Des Moines, Iowa; William K. Brooks, Baltimore, Md.; Charles R. Eastman, Cambridge, Mass.; Henry B. Kummel, Trenton, N. J.; William H. Norton, Mt. Vernon, Iowa; Frank B. Taylor, Fort Wayne, Ind.; Jay B. Woodworth, Cambridge, Mass.

A memorial of James D. Dana, written by Joseph LeConte, was read by H. S. Williams. This was not only an appreciative sketch of Dana's life, but an admirable discussion of the true character of geology as a science, and of the great influence of Dana in giving geology a commanding position.

Other short memorials of Henry B. Nason, Albert E. Foote and Antonio del Castillo were read.

A message of regard was voted to J. P. Lesley, who was unable to attend the meeting on account of illness.

The Society held a morning and an afternoon session on Friday and a morning session on Saturday. It was announced that the next summer meeting, to be held in August in connection with the American Association for the Advancement of Science, would be devoted chiefly to excursions.

The list of papers read was not as long as at the Baltimore meeting, but the program was of excellent quality. Following are the titles of the papers presented:

George P. Merrill, Disintegration and decomposition of diabase at Medford, Mass.; Charles R. Keyes, The geographic relations of the granites and porphyries in the eastern part of the Ozarks; J. F. Kemp, Illustrations of the dynamic metamorphism of anorthosites and related rocks in the Adirondacks; N. S. Shaler, The importance of volcanic dust and pumice in marine deposits; L. V. Pirsson, A needed term in petrography; John J. Stevenson, The Cerrillos coal field of New Mexico; N. S. Shaler, The relations of geologic science to education. (Presidential address), ; W. M. Davis, Note on the outline of Cape Cod; W. M. Davis, Plains of Marine and subaërial denudation; F. P. Gulliver, Cuspate forelands; M. R. Campbell, Drainage modifications and their interpretation; N. H. Darton, Some fine examples of stream robbing in the Catskill Mountains; Robert Bell, Proofs of the rising of the land around Hudson Bay; C. R. Van Hise, Movements of rocks under deformation; Alfred C. Lane, Possible depth of mining and boring; Harry Fielding Reid, Notes on glaciers; Frank Leverett, The relation between ice lobes, south from the Wisconsin driftless area; Frank Leverett, The loess of western Illinois and southeastern Iowa; G. Frederick Wright, High level terraces of the middle Ohio and its tributaries; H. L. Fairchild, Four great kame areas of western New York; Warren Upham, Preglacial and postglacial channels of the Cuyahoga and Rocky Rivers; C. H. Hitchcock, Paleozoic terranes in the Connecticut Valley; C. Willard Hayes, The Devonian formations of the southern Appalachians; N. H. Darton, Notes on relations of lower members of costal plain series in South Carolina; N. H. Darton, Resumé of general stratigraphic relations in the Atlantic costal plain from New Jersey to South Carolina; T. C. Chamberlin, The Natchez formations; Arthur Keith, Some stages of Appalachian erosion.

The American Psychological Association met at the University of Pennsylvania, Philadelphia, Friday and Saturday December 27 and 28, 1895.

Friday, December 27, 10 *A. M.*—Psychology and Physiology, Professor George S. Fullerton; Description of a Series of Physical and Mental Tests on the Students of Columbia College, Dr. Livington Farrand; Some Psycho-Neural Data, Dr. Arthur MacDonald; An Experimental Investigation of the Processes of Ideation, Mr. Oliver Cornman. (Introduced by Professor Lightner Witmer).

2.30 *P. M.*—Address of the President, Professor J. McKeen Cattell; Consciousness and Time, Professor Charles A. Strong; Some Conditions of Will Development, Brother Chrysostom; A Psychological Interpre-

tation of the Rules of Definition in Logic, Professor Alfred H. Lloyd.

Saturday, December 28, 10 *A. M.*—Discussion on Consciousness and Evolution, Professors William James, E. D. Cope, J. Mark Baldwin and G. S. Ladd.

2.30 *P. M.*—An Experiment on the Effects of Loss of Sleep, Professor G. T. W. Patrick; Further Researches on the Psychic Development of Young Animals, and its Physical Correlation, Professor Wesley Mills; Variations in the Patellar Reflex as an Aid in Mental Analysis, Professor Lightner Witmer; Experiments on Induced Hallucinations, Professor James H. Hyslop; A Case of Dream Reasoning, Professor W. Romaine Newbold.

Informal communications were made at various times during the sessions.

A fuller account of the papers and discussions will be found in our department of Psychology; q. v.

Indiana Academy of Science.—The eleventh Annual Meeting of the Indiana Academy of Science was held at Indianapolis, December 27th and 28th.

The session was of unusual interest and the attendance good. Forty two new members were elected. This indicates the interest that is being aroused in the State in scientific lines.

The address of the retiring President, A. W. Butler, of Brookville, on "Indiana: A Century of Changes in the Aspects of Nature," met with enthusiastic applause.

A poem on the "Naturalist" recited by W. W. Pfrimmer was a novel, yet enjoyable feature.

The report of the biological Survey of Turkey Lake was another new feature of the meeting, and attracted much favorable attention.

The following papers were presented:

Unconscious Mental Cerebration, C. E. Newlin; Human Physiology in its Relation to Biology, Guido Bell; A means of preventing Hog Cholera, D. W. Dennis; The Hopkins Seaside Laboratory at Pacific Grove, Cal., B. M. Davis; Glacial and Eolian Sands of the Iroquois and Tippecanoe River Valleys, A. H. Purdue; The recent earthquakes east of the Rocky Mountains, A. H. Purdue; Some minor processes of Erosion, J. T. Scoville; Kettle Holes at Maxinkuckee, J. T. Scoville; Fossils from sewer trenches in the Glacial Drift, Wm. M. Whitten; Relief map of Arkansas, John F. Newsom; Notes on the Fauna of the black shales of Bartholomew and Jackson Counties, V. F. Marsters; Botanical Literature of the State Library, John S. Wright; Microscope slides of vegetable material for use in Determinative work, John

S. Wright; Embryology of *Hydrastis canadensis*, Geo. W. Martin; Some determinative factors underlying Plant Variation, Geo. W. Martin; Variations in the cleavage of the *Fundulus* Egg, Geo. W. Martin; Hæmoglobin and its Derivatives, A. J. Bigney; Effects of heat upon the Irritability of Muscle, A. J. Bigney; The evolution of sex in *Cymatogaster*, C. H. Eigenmann; The circulation of protoplasm in the manubrium of *Chara fragilis*, D. W. Dennis; A new Subterranean Crustacean from Indiana, W. P. Hay; A peculiar crawfish from southern Indiana, W. P. Hay; A note on the breeding habits of the cave salamander, *Speterpes maculicaudus*, W. P. Hay; Notes on a collection of fishes from Dubois County, Indiana, W. J. Moenkhaus; The geographical variation of *Etheostoma nigrum* and *E. olmstedi*, W. J. Moenkhaus; A revision and synonomy of the *Parrus* group of *Unionidæ*, with 6 plates, R. Ellsworth Call; The fishes of the Missouri River Basin, B. W. Evermann and J. T. Scoville; Recent investigations concerning the Redfish (*Oncorhynchus nerka*) at its spawning grounds in Idaho, B. W. Evermann and J. T. Scoville; Additional notes on Indiana birds, A. W. Butler; A mammal new to Indiana, A. W. Butler; Some beneficial results from the use of Fungicides as a preventive of Corn Smut, Wm. Stuart; Ratio of alcohol to yeast in Fermentation, Katherine E. Golden; Distribution of *Orchidaceæ* in Indiana, Alida M. Cunnigham; A new station for *Pleodorina*, Severance Burrage; Additional notes on Animal Parasites collected in the State, A. W. Bitting; Report upon certain collections presented to State Biological Survey, Stanley Coulter; Infection by Bread, Katherine E. Golden; Certain plants as an index of Soil Character, Stanley Coulter; Forms of *Xanthium canadense* and *X. strumarium*, J. C. Arthur; A new habitat for *Gastrophilus*, A. W. Bitting; Noteworthy Indiana Phanerogams, Stanley Coulter.

The following reports relating to the State Biological Survey were made:

Second contribution to the knowledge of Indiana Mollusca, R. Ellsworth Call; Contributions to the Biological Survey of Wabash County, Albert B. Ulrey; Report of the Biological Survey, Zoölogy, C. H. Eigenmann.

Turkey Lake has been taken as a station for exhaustive study of a limit of environment and the variation of its inhabitants, and the following reports represent the first seasons work:

First Report of the Biological Station, C. H. Eigenmann; Some of the physical features of Turkey Lake, D. C. Ridgley; Hydrographic map of Turkey Lake, J. Juday; Temperatures of Turkey Lake, J. P.

Dolan; Inhabitants of Turkey Lake in general, C. H. Eigenmann; *Hirudinea* of Turkey Lake, Bessie C. Ridgley; *Rotifera* of Turkey Lake, D. C. Kellicott; *Clodocera* of Turkey Lake, E. S. Birge; *Mollusca* of Turkey Lake, R. Ellsworth Call; *Odonata* of Turkey Lake, D. C. Kellicott; Fishes and tailed batrachians of Turkey Lake, C. H. Eigenmann; Tailless batrachians of Turkey Lake, C. Atkinson; Snakes of Turkey Lake, H. G. Reddick; Turtles of Turkey Lake, C. H. Eigenmann; Water birds of Turkey Lake, N. M. Chamberlain; Flora of Turkey Lake, O. H. Meincke; Methods of determining Variations, C. H. Eigenmann; Variation of *Etheostoma* of Turkey and Tippecanoe Lakes, W. J. Moenkhaus.

The officers for the next year are as follows:

President, Stanley Coulter of Purdue University; Vice-President, Thos. C. Gray of Rose Polytechnic; Secretary, John S. Wright of Indianapolis; Assistant Secretary, A. J. Bigney of Moores Hill College; Treasurer, W. P. Shannon of Greensburg.

A. J. Bigney, *Assistant Secretary.*

The Biological Society of Washington.—November 30th, the following communications were read: Edw. L. Greene, Some Fundamentals of Nomenclature; Theo. Holm, Contributions to the flora of the District of Columbia; David White, The Mode of Development of Exogenous Structure in Paleozoic Lycopods, a review of Williamson and Renault.

SCIENTIFIC NEWS.

Notice Concerning the Geological Map of Europe, Published Under the Auspices of the International Congress of Geologists.—At the Third Session of the International Congress of Geologists, held in Berlin in 1885, the committee on a geological map of Europe made a report, in which the following conditions of publication were announced (Berlin Volume, page LXII): "The house of Reimer & Co., undertakes the publication at its own expense on the sole condition that the international committee guarantee the sale of 900 copies at 100 francs per copy, and furnishes the sum in advance.

The subscription price of 100 francs will be augmented to 125 francs in the regular book trade.

The committee has divided this guarantee subscription as follows: Each one of the large countries of Europe (to wit: Great Britain, France, Spain, Italy, Austro-Hungary, Germany, Scandinavia and Russia) agrees to take 100 copies. The six small countries (i. e., Belgium, Holland, Denmark, Switzerland, Portugal, Roumania) will divide among them the remaining 100 copies, etc."

In the Fourth Session of the Congress held in London in 1888, the following note occurs in the report of the proceedings of the committee on the geological map of Europe (London Volume, p. 59).

"The American committee requested of the Directory to be admitted as a subscriber to the map of Europe on the same terms as the great countries of Europe ('sic') i. e.. for at least one hundred copies and at the same price."

Dr. Frazer, the Secretary of the American Committee, obtained the names of American subscribers to the "one hundred copies at the same price" (100 francs), within a short time of the granting of this request, and promptly notified the publication committee in Berlin, Messrs. Beyrich and Hauchecorne, of the fact.

It appears, however, the map is being offered for sale in the German catalogues at the price mentioned in the Berlin resolution as that accorded to original subscribers.

On this account the undersigned advises the survivors of those who so patriotically came forward in 1888 to enable the geologists of the United States to enjoy same privileges as those of the great countries of Europe, to send through their own agents for the geological map of Europe, since there would no longer be any advantage in obtaining them through a single channel.

List of subscribers to the geological map of Europe in the order of their subscriptions, with number of copies:

Williams College, 1; Ohio State Univ., Columbus, 1; Rensselaer Polytechnic Institute, 1; University of Virginia, 1; Am. Inst. of Mining Engineers, 1; Amherst College, 1; Cornell University Library, 1; Provincial Museum, Halifax, 1; Wesleyan University, Middletown, Conn., 1; Lehigh University, Bethlehem, Pa., 1; Academy of Natural Sciences, Philadelphia, 1; Univ. of California, Berkely, Cal., 1; Prof. C. H. Hitchcock, for Dartmouth College, 1; Prof. J. S. Newberry (dead), 1; Indiana University, 1; Smith College, Northampton, Mass., 1; U. S. Geological Survey, Washington, D. C., 3; Rutgers College, New Brunswick, N. J., 1; Yale University Library, 1; American Geographical Society, 1; Peter Redpath Museum, McGill College, Montreal, 1; U. S. Military Academy, West Point, N. Y., 1; Prof. G.

A. König, 1; N. Y. State Library, Capitol, Albany, 2; Eckley B. Coxe, Drifton, Pa. (dead), 2; University of Nebraska, 1; Kansas State Library, 1; B. S. Lyman, 1; Johns Hopkins University, 1; F. W. Matthieson, La Salle, Ill., 1; Lehigh Valley R. R. Co., Philadelphia, 1; E. V. d'Invilliers, Philadelphia, 1; University of Wisconsin, Madison, Wis., 1; Second Geological Survey of Pennsylvania, 2; State Mining Bureau of California, 1; Washington University, 1; Dr. R. W. Raymond, 1; Franklin Institute, Phila., 1; Harvard College Library, 1; University of North Carolina, Chapel Hill, 1; University of the City of New York, 1; Massachusetts Agric. College, Amherst, 1; W. S. Keyes, San Francisco, Cal., 1; R. D. Baker, Philadelphia, 2; S. F. Emmons, U. S. Geological Survey, Washington, D. C., 1; H. M. Sims, Shenandoah, Page Co., Va., 1; American Museum of Natural History, N. Y., 1; Prof. Alexander Winchell, Univ. of Mich., Ann Arbor (dead), 1; H. Huber, Argentine, Kansas, 1; Jas. E. Mills, E. Quincy, Cal., 1; Cooper Union, N. Y., 1; Collegiate and Polytechnic Institute, Brooklyn, 1; Cornell University, N. Y., 1; Joseph D. Potts, Philadelphia (dead), 1; Prof. J. C. Fales, Danville, Ky., Centre College, 1; T. H. Aldrich, Blocton, Ala., 1; Chas. Paine, Pittsburg, 1; Colorado School of Mines, Golden, Col., 1; Western Reserve Univ. (d. E. W. Morley), Cleveland, Ohio, 1; F. Klepotoko, Houghton, Michigan, 1; Thos. Macfarlane, Ottawa, Canada, 1; Arkansas Geological Survey, Little Rock, 1; Buchtel College, Akron, Ohio, 1; Mercantile Library, Philadelphia, 1; University of Michigan, Ann Arbor, 1; Alabama Geological Survey, University of Alabama, 1; E. S. Whelen, Philadelphia (dead), 1; Worcester Polytechnic Institute, 1; Julius Bien, N. Y., 1; W. A. Ingham, 1; Dr. Jas. P. Kimball, 109 East 15th St., N. Y. City, 1; Dr. J. S. Newberry, N. Y., Dec. 29, '87, 1; New Harmony Institution, Ind., 1; R. Ellsworth Call, Des Moines, Iowa, 1; Bost. Soc. Nat. Hist., 1; Hastings, Jno. B., Ketchum, Alturas Co., Idaho, 1; Geol. Surv. of Minn., Minneapolis, Minn., 1; Lacoe, R. D., Pittston, Luzerne Co., Penna., 1; Vassar College, Poughkeepsie, N. Y., 1; Mt. Holyoke Seminary, South Hadley, Mass., 1; Colby University, Waterville, Me., 1; Cincinnati Soc. of Nat. History, 1; Packer Collegiate Institution, Brooklyn, N. Y., 1; Emmens, Stephen H., Harrison, N. Y., 1; School of Mines, Rapid City, Dakota Territory, 1; Ohio University, Athens, O., Prof. A. D. Morrill, 1; Proctor, John R., Franklin, Ky., Aug. 19, '88, 1; Rose Polytechnic School, Terre Haute, Ind., Aug. 19, '88, 1; Read, Jas. P., Calico, San Bernard Co., Aug. 31, '88, 1; Oberlin College, Ohio, Aug. 23, '88, 1; Frazer, Persifor, Philadelphia, 1; Streator Township High School, La Salle Co., Ill., R. Wil-

liam Brice, Sept. 21, '88, 1; State Univ., Athens, Ga., Prof. J. W. Spencer, Nov. 12, 1888, 1; Lowry, Thos., Minneapolis, Minn., Nov. 13, 1888 (N. H. Winchell), Nov. 13, 1.—Total, 100.

Dr. Eugenio Dugès died in Morelia, Mex., Jany. 13th. 1895. He was born in Montpellier, France, but had resided in Mexico since 1865. He was a special student of Coleoptera, and had furnished students in the United States with many specimens.

Dr. Adolf Gerstäcker, Professor of Zoology in the University of Griefswald, died June 20th, 1895. He was born Aug. 30th, 1828, and is widest known from his share in the Zoologie of Carus and Gerstäcker and his contributions to Bronns' Thierleben.

Dr. Th. Ebert has been called as Professor of Paleontology to the Prussian Geological Institute, and Dr. Müller as Professor of Regional Geology in the same institute.

Henry John Carter, well known for his researches on Protozoa, Sponges, etc., died at Rudleigh Salterton, England, May 4th, 1895.

Dr. Wm. H. Flower, of the British Museum, has been elected corresponding member for anatomy of the Paris Academy of Sciences.

Dr. W. I. Nickerson, of the University of Colorado, has been appointed Instructor in Biology in the University of Evanston, Ill.

Prof. A. Sabatier, of Montpellier, has been elected corresponding member for Zoology of the Paris Academy of Sciences.

Dr. F. Schütt, of Kiel, has been appointed Professor of Botany and Director of the Botanical Gardens at Griefswald.

Dr. Joseph G. Norwood, the well-known geologist and paleontologist, died at Columbia, Mo., May 6th, 1895.

Dr. E. Hering, of Prague, becomes Professor of Physiology at Leipzig, as successor to the late Prof. Ludwig.

Dr. René duBois Raymond is assistant in the experimental division of the Physiological Institute in Berlin.

Dr. W. A. Setchell, of Yale College, has been appointed Professor of Botany in the University of California.

Dr. F. Sansoni, Professor of Mineralogy in Pavia, and editor of the Italian Journal of Mineralogy, is dead.

Mr. Charles D. Aldright has been appointed Instructor in Biology at the University of Cincinnati.

C. C. Babington, Professor of Botany in the University of Cambridge, died July 22d, aged 86.

James Mortimer Adye, an entomologist, died at Bournemouth, England, May 30th, 1895, aged 34.

Dr. Jas. E. Humphrey has been appointed Lecturer in Botany in Johns Hopkins University.

Dr. A. Kowalevsky has been elected a foreign associate of the Academy of Sciences of Paris.

Prof. J. G. Agardh has given his magnificent collection of Algæ to the University of Lund.

Pietro Doderlein, Professor of Zoology and Geology in Palermo, died March 28, aged 84.

Dr. Pellegrino Strobel, geologist and Conchologist, died at Parma, Italy, June 9th, 1895.

Dr. R. Hanitsch has gone as Director to the Raffles Museum and Library at Singapore.

The Linnean Society of London has awarded a gold medal to Prof. F. Cohn, of Breslau.

Dr. Gustav von Nordenskiold, ethnographer and crystallographer, of Stockholm, is dead.

Dr. A. D. Mead has been appointed Instructor in Neurology in Brown University.

Dr. Reinitzer, of Prag, has been called as Extraordinarius Professor of Botany to Graz.

Dr. E. Schöbl succeeded Dr. Schiemenz as Librarian of the Naples Zoological Station.

Prof. E. D. Cope has been elected associate member of the Academy of Sciences, Arts and Letters of Belgium.

Dr. R. Bonnet, of Giessen, goes as Professor Ordinarius of Anatomy to Griefswald.

Dr. W. Roux, of Innspruck, has gone as Ordinary Professor of Anatomy to Halle.

Dr. Hans Schinz is appointed Ordinary Professor of Botany in Zürich.

Julien Deby, of London, microscopist and student of diatoms, is dead.

Dr. F. C. Kenyon has gone to Clark University as Fellow in Biology.

Dr. H. Lenk, of Leipzig, has made Extraordinarius of Geology.

PROSPECTUS FOR 1896.

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THE AMERICAN NATURALIST

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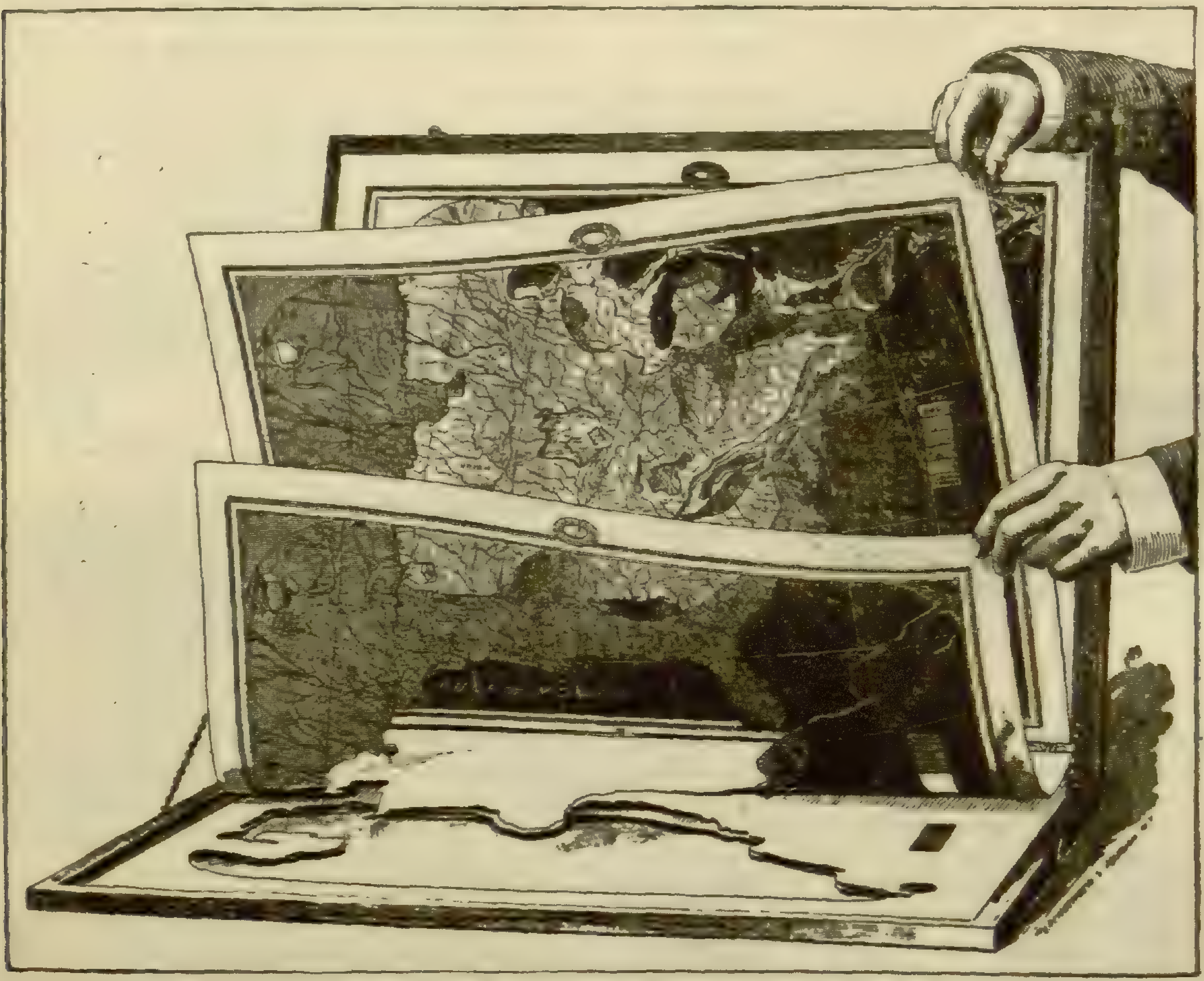

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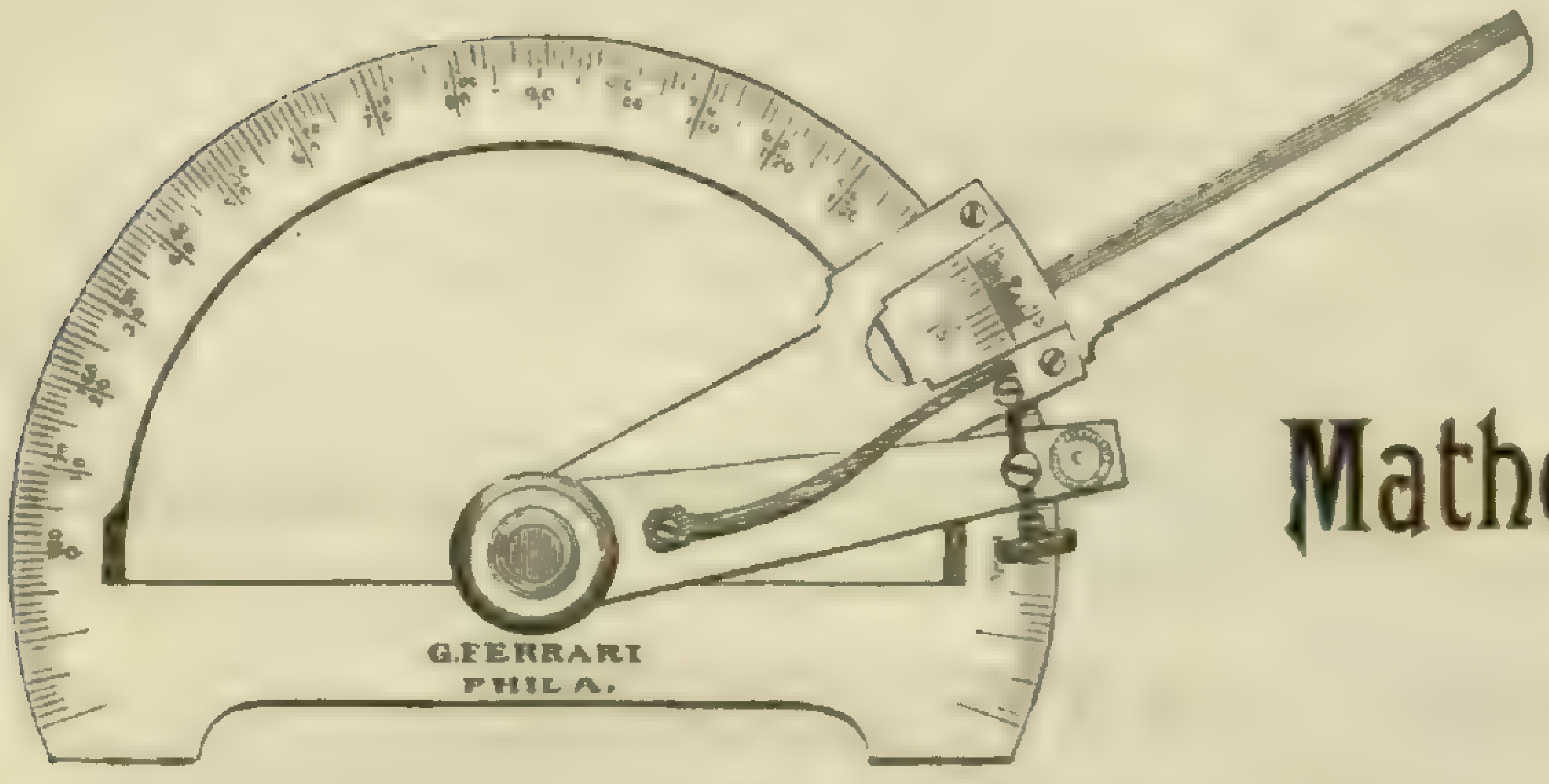

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AMERICAN NATURALIST

VOL. XXX. March, 1896. 351

THE HISTORY AND PRINCIPLES OF GEOLOGY, AND ITS AIM.

BY J. C. HARTZELL, JR., M. S.

From the earliest times the structure of the earth has been an object of interest to man, not merely on account of the useful materials he obtained from its rocky formation, but also for the curiosity awakened by strange objects it presented to his notice. The south and west of Asia, and much of the country bordering the Mediterranean, were particularly favorable for directing attention to geological phenomena. Earthquakes were of frequent occurrence, changing the relative positions of sea and land. Volcanoes were seen in eruption, adding layers of molten rock to those of sand and mud filled with the shells of the ocean. The strata in the hills abounded in evidences of similar collections of vegetable and marine life far removed from access of the sea.

The structure of the earth, however, received but little attention previous to the 7th century, B. C. The extent of the surface known was limited, and the changes upon it were not so rapid as to excite special attention. The ancient Hebrews, in the time of Solomon (1015 B. C.), prosecuted their voyages

through the Straits of Babelmandeb into the Indian Ocean, bringing home the produce of the tropical regions; while the ships sent westward to the Atlantic returned with tin, silver, lead and other metalic products of Spain and Great Britian.

The earliest idea formed of the earth seems to have been that it was a flat circular disk, surrounded on all sides by water, and covered with the heavens as with a canopy, even philosophers looked upon the earth as a disk swimming upon the water. Homer (800 B. C.) regarded the earth as a flat circle surrounded by mysterious waters. The nations that were upon its border were called Cimmerians, and were supposed to live in perpetual darkness.

As the ancients slowly gained a knowledge of the country surrounding their provinces through commercial intercourse, wars, and the search for knowledge, they were undoubtedly struck with the differences of the topography and formations. Thus geology is undoubtedly the outgrowth of geographical knowledge.

The 7th and 6th centuries B. C. were remarkable for great advance in the knowledge of the form and extent of the earth.

Their first discoveries were probably made by the Phœnicians. Their investigations were along the shores of the Mediterranean, and passing through the Straits of Gibraltar, they extended their researches into Spain and Africa and the Canaries.

Pythagoras (583 B. C.) observed the phenomena that were then attending the surface of the earth, and proposed theories for explaining the changes that had taken place in geological time. He held that in addition to volcanic action, the changes in the level of the sea and land were due to the retiring of the sea.

Aristotle (384 B. C.) recognized the interchange constantly taking place between land and sea by the action of running water and of earthquakes, and remarked "how little man can perceive in the short space of his life of operations extending through eternity of time."

Geographical knowledge was greatly advanced by the conquest of Alexander the Great (356 B. C.), in making known

Persia, and science was advanced by sending out expeditions to explore and survey the various provinces he had conquered. The Greeks he sent out, and also those who accompanied him, were critical observers and carefully described the products and aspects of the country, and made collections of all that was interesting in regard to the organic and inorganic products.

Ptolemy (323 B. C.) discovered Abyssinia and navigated the Arabian Sea, and Silineus (306 B. C.) ascended the Ganges to Batna and extended his expedition to the Indus.

It was the military genius of the Romans which led to the survey of nearly all Europe, and large tracts of Asia and Africa. In the height of their power they had surveyed and explored all the coast of the Mediterranean, Italy, the Balkan peninsula, Spain, Gaul, West Germany and Britain, and their practical genius led them to the study of the natural resources of every province and state brought under their sway.

Eratosthenes (276 B. C.) considered the world to be a sphere revolving with its surrounding atmosphere on one and the same axis and having one center. His theories were perfected by Hipparchas (160 B. C.). He attempted to catalogue the stars and to fix their relative position, and he applied to the determining of every point on the surface the same rule he introduced in the arrangement of the constellation.

Strabo (60 B. C.) noticed the rise and fall of the tide, and maintained that the *land* changed its level and not the *sea*, and that such changes happened more easily to the land beneath the sea on account of its humidity.

Ptolemy (150 A. D.) was the first scientific geographer. He followed the principles of Hipparchas, which had been neglected during the two centuries and a half since his time, even by Strabo and Pliny. In Ptolemy's work is found for the first time the mathematical principle of the construction of maps, as well as several projections of the earth's surface.

After the great achievements of Ptolemy to the 13th century, the cultivation of the physical sciences was neglected. In the 10th century Avicenna, Almar, and other Arabian writers commented on the works of the Romans, but added little of their own.

From the 13th to the 16th century, astronomy, travels, and commercial interests occupied the attention of the different nations, but geology did not appear as a separate science until in Italy in the 16th century. It began by being a record of observed facts. This was not enough, however, for it did not satisfy the demand as to how the phenomena were produced. High above sea level, and far inland, imbedded in solid rock, were found fossils. At the outset it was unfortunately linked to the belief that they were relics of the Noachean deluge. Some held that they were the result of the formation of a fatty matter, or of terrestrial exhalations or of the influence of the heavenly bodies, or that they were merely concreations, or sports of nature. The abundance of fossils in the strata of the Apennine range could not fail to arrest attention and excite inquiries. Leonardo da Vinci (1519) and Fracostaro, whose attention was engaged by the multitude of curious petrifactions which were brought to light in 1517 on the mountains of Verona in quarrying rock for repairing the city, had sound views, and showed the inadequacy of the terrestrial deluge to collect marine fossils.

Collections were made for museums, that of Canceolarius, at Verona being the most famous. Descriptive catalogues of these collections were published.

Only a few held that they were the remains of animals. Palissy in 1580, was the first who dared to assert in Paris that fossil remains once belonged to marine animals. The question was naturally asked "How came they there?" The result of investigation showed that the rocks must have accumulated around them, and hence could not always have been as they were found and that the arrangement must have changed since they were formed. This brought about the study of the construction of the earth.

Their chief objects were the examination of the materials out of which the solid framework of the earth was built, and the determination of their chemical composition, physical properties, manner of occurrence, and their characteristics. Thus they started out with the idea that rocks were made through secondary causes.

Steno (1669) observed a succession in the strata, and proposed the theory that there were rocks older than the fossiliferous strata in which organic remains occur. He also distinguished between marine and fluvialite formations. He also published his work "De solido intra solidum naturalites contento," in which he proves the identity of the fossil teeth found in Tuscany with those of living sharks.

Scilla, in 1670, published a treatise on the fossils of Calabria, and maintained the organic nature of fossil shells. But both Steno and Scilla referred their occurrence to the Noachean deluge.

In England the diluvialists were busy forming idle theories to give plausibility to their creed, that the Noachean deluge was the cause of all the past changes on the earth's surface. Differing somewhat in detail, they all agreed in the notion of an interior abyss whence the waters rushed, breaking up and bursting through the crust of the earth, to cover the surface, and whither, after the deluge, they returned. Such absurd notions greatly hindered the advance of science.

Leibnitz (1680) proposed the bold theory that the earth was originally in a molten state from heat, and that the primary rocks were formed by the cooling of the surface, which also produced the primeval ocean by condensing the surrounding vapors. The sedimentary strata, he held, resulted from the subsiding of the waters that had been put in motion from the collapse of the crust on the cooling and contracting nucleus.

Burnet (1680) published his "Sacred Theory of the Earth," and it received great applause. It was written in ignorance of the facts of the earth's structure, and was an ingenious speculation. It abounds in sublime and poetical conceptions in language of extraordinary eloquence. In 1692 he published a work which treated of the Mosaic Fall as an allegory.

Lister sent to the Royal Society, in 1683, a proposal for maps of salts and minerals. He was the first to recognize the arrangement of the earth's materials in strata, continuous over large areas, and resembling each other in different countries.

Hooke (1688) and Ray (1690), differing as much from Burnet as from Leibnitz, considered the essential condition of the

globe to be one of change, and that the forces now in action would, if allowed sufficient time, produce changes as great as those of geological time. Hooke published a "Discourse on Earthquakes," which contains the most philosophical view of the time respecting the notions of fossils and the effect of earthquakes in raising up the bed of the sea. Woodward perceived that the lines of outcrops of the strata were parallel with the ranges of mountains. He formed, about the year 1695, a collection of specimens which he systematically arranged and gave to the University of Cambridge.

They were followed in the same direction by Vallismeme (1720), Moro (1740,) Buffon (1749), Lehman (1756), and Fuchsel (1773), each contributing something additional, and advanced the most philosophical views yet presented respecting the fossiliferous strata. The first two made observations throughout Italy and the Alps. Moro endeavored to make the production of strata correspond in time with the account of the creation of the world in six days.

Buffon published his "Natural History," in which he advanced views respecting the formation and modification of mountains and valleys by the action of water.

Geology did not begin to assume the rank of an important science until its application to the practical purposes of mining and agriculture was first pointed out in 1780 by Werner, Prof. of Mineralogy in the School of Mines at Freiberg in Saxony. He greatly advanced the science by establishing the superposition of certain groups, by giving a system and names. He had very crude ideas regarding the origin of the strata. He supposed that the various formations were precipitated over the earth in succession from a chaotic fluid; even the igneous rocks he held to be chemical precipitations from the waters.

Thus we see that the history of geology has been a record of failures, and it was not until Hutton (1788), rejecting all theories as to the beginning of the world returned to the opinions of Pythagoras and Ray. He pointed out that geologists must study the *present* if they would learn of the *past*; and he labored to show that the forces now in operation are capable

of forming rocks and of bringing about the changes that have occurred on the earth. He held that the strata which now compose the continents were once beneath the sea, and were formed out of the waste of preëxisting continents by the action of the same forces which are now destroying even the hardest rocks. Hutton was the kind of man the science had so long been in need of, and by his teaching geologists were at last started on the only path that could possibly lead them to truth. He drove out at once and forever the imaginary agencies which the early geologists had been so ready to have recourse to, and laid down the principle that in geological speculation "no powers are to be employed that are not natural to the globe, no actions to be admitted of except those of which we know the principle, and no extraordinary events to be alleged in order to explain a common appearance." He occupied himself mainly studying the changes that are now taking place on the earth's surface, and the means by which they were brought about, and in demonstrating the fact that the changes that had happened during the past periods of the earth's history were of the same kind and due to the same causes as those now going on.

The determination of the order of the strata, and the grouping of them in chronological order, were begun by Lehman (1756) and carried on by Fuchsel (1773), Pallas (1785) and Werner (1789). Smith made the most important contribution to this subject when, in 1790, he published his *Tabular View of the British Strata*. He showed their superposition and characterized the different groups by their peculiar fossils.

(*To be continued.*)

THE CONSTANCY OF BACTERIAL SPECIES IN NORMAL FORE MILK.[1]

BY H. L. BOLLEY.

It is recognized that aside from actual dirt, as, for example, drippings from the hands of the milker, dirt from his clothing, and hairs and manurial particles from the sides of the animal, that the fore milk constitutes the most productive source of the bacterial flora of milk. Schultz and others have placed quantitative determinations at from fifty to one hundred thousand per cubic centimeter. As the character of the germ content is becoming such a matter of importance in economic labors with milk and its product, it is apparent that a consideration of the types of germ present in the normal udder should command early attention of the bacteriologically inclined dairymen.

The question is of necessity, one of such breadth that it must be approached in separate phases, such, as for example, the study of the presence or absence of physiological groups, constancy of definite species, etc. During the year just closed two such points have been under investigation. The primary object, while being a matter of simple interest, had also the direct aim of determining the relation of normal fore milk to curd inflation in cheese manufactory. The results of the work have in part been reported in a paper read before the General Section of the American Association of Agricultural Colleges and Experimental Stations, July 19, 1895; showing that, in so far as the investigation had been carried, gas generating species such as are accountable for "pinhole formation" or curd inflation are not normal to the fore milk of the healthy udder.[2]

[1] Read before the Section of Botany of the American Association for the Advancement of Science, Springfield Meeting, August 31, 1895. Also published in Centralblatt für Bacteriologie und Parasitenkunde, Ab. II, B and I, No. 22-23.

[2] Bolley and Hall: Cheese curd inflation: Its relation to the bacterial flora of fore milk. Centralb. f. Bact. u. Parasiteuk., II, Ab. I, Bd., No. 22-23.

This conclusion was based upon preliminary cheese curd tests made at Madison, Wisconsin, August, 1894, and duplicated at Fargo in October, and finally upon qualitative analysis made during a period of three winter months, with ten different milch cows under consideration.

The point to be reported upon, at this time, is that of the constancy of species as found : (a) for the same cow for a given length of time ; (b) in the same teat of the same cow ; and (c) as to whether species are common to different cows or not upon same dates.

In general, the evidence of the work associated with the last named report, was to the effect that there is no evidence that germs are of any certainty common to different animals upon the same date under like conditions ; but that a certain inhabitant of the udder of the same animal may remain quite constant. Thus while only one species, number 30, was observed to be present in more than two animals of the original ten animal test upon different dates, several different species were found to occur at several dates in the same udder.

Commencing July 1st, three animals were placed under cultural investigation, number 24 of which was an animal of the original ten, also number 21. Cultures were attempted from each teat upon gelatine and agar, as often as the work could be handled, the same methods of procuring milk being used as in the previous work, except in the different tests of the same animal, the milk tube or trochar, was inserted different depths. Some sixty of these distinct milkings were taken upon fifteen different dates, during which time the cows ran upon a clean pasture during the day, being housed at night. The milk samples were taken sometimes in the morning and sometimes at night. In all, thirty-seven different species of bacteria were separated ; and, as in past work, were found to be of various physiological types, gelatine liquifiers, non-liquifiers, solid curd types, peptonizing forms, acid and alkali curdlers, etc., including bacilli, micrococci of various forms, and a streptococcus. Thus it may be said that, in general, forms collected are miscellaneous.

Results: Again, there is no marked evidence that species are common among different animals, but there is strong evidence of constancy of appearance of certain types when once present. This, perhaps, is to be expected, for it is hardly possible that in an ordinary milking all individuals could be excluded from the milk cistern and lower teat passages.

The following table and annotations may help to show the bearings of the work:

Cow No. 24	Species present, per teat, by dates.			
Teats =	No. 1	No. 2	No. 3	No. 4
*Expr. No. 1, July 2nd.	Nos. 1	Nos. 1	Nos. 1	Nos. 5
Expr. No. 2, July 3rd.	6	1	9 and 10	5, 100 & 77
Expr. No. 3, July 4th.	16	1	15	(Not taken)
Expr. No. 4, July 6th.	(Not taken)	17 and 1	20	20
Expr. No. 5, July 8th.	(Lost Cul.)	23	10, 61	26, 27, 15, 29
Expr. No. 6, July 10th.	30, 1	(Lost)	31	(Not taken)
Expr. No. 10, July 17th.	58, 53, 1	1	61	66, 20, 15, 1
Expr. No. 13, July 23rd.	96, 93, 94	1	96, 97	20, 11, 100, 1
Expr. No. 15, July 28th.	77, 67	(Not taken)	66, 100 & 67	67, 1

*The numbers in each columns—1, 2, 3 and 4 = the laboratory numbers given the different species.

Annotation No. 1, a solid curd, lactic acid forming micrococcus, is seen to be present upon every date, appearing in teat No. 2 upon all possible dates save one.

Nos. 5, 10, 15, 61 and 67 occurred twice each, the intervening days being respectively 2, 8, 7, 4 and 4. It is worthy of note that with the exception of No. 67, each of these was found each time in the same teat.

Cow No. 21	Species present, per teat, by dates.			
Teats =	No. 1	No. 2	No. 3	No. 4
Expr. No. 8, July 12th.	Nos. 45	Nos. 31	Nos. 27, 31	Nos. 20
Expr. No. 9, July, 15th.	(Lost)	31, 50	29, 53	55, 56, 57, & 31
Expr. No. 12, July 16th.	53, 51, & 56	31, 45	(Not taken)	(Lost)

Annotations:—With this animal it is to be noted that No. 31, a lactic acid forming micrococcus, is constant to all dates, and upon each date was found present in teat No. 2.

Other germs found twice each were Nos. 45, 53 and 56; but each time in a different teat.

Cow No. 26	Species present, per teat, by dates.			
Teats =	No. 1	No. 2	No. 3	No. 4
Expr. No. 7, July 1st.	33, 1	33	39, 61, 67	17, 44, 33
Expr. No. 17, July 17th.	66, 100, 67 17 and 33	33, 15	33	53, 77
Expr. No. 14, July 23rd.	33	67, 33	33	(Lost)

In these three milkings from cow No. 26, the common species to each date are seen to be Nos. 33 and 67. Out of eleven milk samples taken No. 33 occurs in the cultures nine times. The intervening dates being 16, 6 and 22 days apart. No. 33 is a streptococcus and in these distant tests, as to time separation, is a strong argument of constancy of presence being possible to an individual species. In growth characteristics this germ is almost a strict anaerobe.

Studying these tables, we find for each animal the following numbered germs present:

Cow No. 24.—Nos. 1, 5, 6, 9, 10, 100, 77, 15, 16, 17, 20, 23, 61, 26, 27,.29, 30, 31, 58, 53, 66, 96, 93, 94, 97, 11 and 67, a total of twenty-seven distinct forms.

Cow No. 21.—Nos. 45, 31, 27, 20, 50, 29, 53, 55, 56, 57 and 51, a total of eleven.

Cow No. 26.—Nos. 33, 1, 39, 61, 67, 17, 44, 66, 100, 15, 53 and 77, a total of twelve.

The forms common to three animals equal only one, No. 53, while those common to two of them are seen to be Nos. 1, 100, 77, 15, 61, 29, 31, 66, 67, 29 and 20; eleven constant forms.

General Annotations:—From these summaries it is to be noted that cow No. 24 from nine different milkings furnished twenty-seven of the thirty seven germs of the three tests, *cow*

No. 21 six and cow No. 26 four. The numbered germs from the last named animals are representative of but three milking dates each. It is thus a possibility, that further milking dates for these cows might have given others of those common to cow No. 24. While this point last named, is probably a correct consideration, it is nevertheless quite clearly indicated that the great majority of germs are but incidental in a given udder or teat to the date, perhaps, to the environments of the animal. There are, however, certain few germs found which when once present in a teat or udder, remain with marked presistence. For this capability, these are found to possess what are presumably the proper physiological functions or requirements, as for example, capability to properly thrive in or withstand the normal temperature of the animal's body, and anaerobic or semi-anaerobic faculties.

As in the case of the paper previously mentioned, this is given not as final evidence to convince upon the points mentioned or suggested, but rather as a record of preliminary work accomplished.

Again, an interesting fact is the comparatively low number of species per milk sample. In the first work, winter collections, the range was from one to four species, in this it is one to five with a rather high average number. It is also interesting, though perhaps to be expected, that quantitative determinations vary from low to high numbers for different milkings, very much in accord to these last named figures.

North Dakota Experiment Station, Fargo, N. D., August, 20, 1895.

LIFE BEFORE FOSSILS.

By Charles Morris.

The beginning of life upon the earth is one of those mysteries which, to judge from what we now know about it, seems likely never to be solved by ascertained facts. There are mod-

ern facts, indeed, which bear upon it, but few geological ones, and none of absolute force. If we leave out of the question the highly problematical "Eozoon Canadense," we find the first known fossils at a comparatively high level in the rocks; and these, instead of being, as the theory of evolution requires, of very simple organization, are of a degree of development which indicates a very long period of preceding life existence. This primeval fauna, indeed, contains representatives of every branch of animal life except the vertebrate, and these not in their simplest stage, but already divided into their principal orders: the Cœlenterate class, for instance, yielding examples of Actinozoa and Hydrozoa; the Crustacean, of Trilobites and Phyllopods; and the Molluscan, of Gasteropods, Lamellibranchs and Pteropods.

This is the beginning of life as we know it. It is very far from the beginning of life as evolution demands, or as the character of the rock strata indicates. Below the Lower Cambrian beds, which contain these fossils, lie several miles of stratified rocks similar in physical character to those above them, and indicating, as Darwin says, "that during a preceding era as long as, or probably far longer than, the whole interval from the Cambrian age to the present day . . . . the world swarmed with living creatures."

Evidently we are not yet at the origin of life. We are miles away from it probably—miles of rock strata, that is. Between the simplest known microscopic creatures and the much developed Cambrian fossils an immense gap extends. The gap, for example, between a diatom and an oyster is one that represents ages of evolution; yet it is much less in extent than the yawning gap which we find dividing the line of primeval life, and which geologists have sought in vain to fill. Believers in evolution—who represent about all living scientists and the bulk of living thinkers—cannot but stand in some dismay before this strange circumstance, which must be proved away or explained away before their theory can be fully substantiated. Yet proof is not forthcoming, and only attempts at explanation remain.

In April, 1885, I presented certain views on this subject before the Academy of Natural Sciences of Philadelphia, and reinforced my arguments by later communications in 1885 and 1886. In 1894 Professor W. K. Brooks, evidently unaware of the existence of the papers mentioned, advanced a similar hypothesis in the July–August number of the "Journal of Geology," presenting a number of interesting facts, though missing, as it seems to me, much the strongest argument in defence of the hypothesis.

I propose here to repeat my former hypothesis, with additional arguments and illustrations—for some of the latter of which I acknowledge indebtedness to Professor Brooks's able paper.

To begin with, the facts of embryology may be said to point directly to what was probably the primary condition of life. The embryos of ocean animals, as a rule, begin life as swimming forms. Even the oyster—a type of sluggishness in animals—enjoys a brief existence as a swimmer before it acquires a shell and becomes permanently fixed. The same is the case with the sponge, the coral, and other stationary types, and with the various creeping or slow moving forms, such as the echinoderms. Since it has become a settled dogma of science that each stage of development passed through by the embryo represents some mature stage in the ancient ancestry of the animal, the fact stated points almost irresistably to the conclusion that the far off ancestors of the present stationary or crawling animals were swimmers—and, for that matter, naked swimmers, they being as yet destitute of hard skeletal parts.

Yet no swimming stage of existence is indicated by the oldest known fossils, or at least only by the minute pteropods and phyllopods, which were, perhaps, secondary derivatives from crawling ancestors. The trilobite may have had some swimming powers, yet probably made its way only by crawling, and the other known forms were crawlers or burrowers, or were immovably fixed. There are traces of jelly fish, it is true, but these, as they now exist, we know to be derivatives from stationary forms, and the primeval swimmers indicated by embryology have left no trace of their existence in the rocks.

Yet the oceanic waters to-day swarm with swimming life, and in all probability did so then. This life, as now existing, contains many high as well as numerous low forms. Then it must have consisted of low forms only. The wealth of existing minor sea life, as observed by the unassisted eye and revealed by the microscope, is simply boundless. Small jelly fish are met with in vast armies, hundreds of miles in extent, and descending to many feet in depth. Pteropods, both the naked and the shelled forms, occur in prodigious multitudes. The minute copepod crustaceans are found in countless swarms, and, though consumed in myriads daily by herring and other fish, by medusæ, siphonophora and other invertebrates, and even by the whale, they are so productive that their numbers seem undiminished, being found over vast areas of surface and extending through more than a mile in vertical depth. Below these again are hosts of microscopic larvæ and minute animals, and still lower are countless swarms of protozoa, such as radiolarians, globigerinæ, etc.

Here, then, are innumerable swarms of swimming and floating forms, in most part carnivorous, but necessarily requiring a vegetable basis of nutriment. The foundation food supply for such a mighty host must be enormous in quantity. The visible plant life of the ocean, the algæ which grow on the bottom, would not sustain a tithe of such an army. The microscope must again be brought into requisition, and this useful instrument reveals to us an extraordinary profusion of unicellular plants—diatoms, coccospheres, trichodesmiums, and a few other types—which extend from the surface to the lowest level of light penetration, and are so extraordinarily numerous and prolific as to supply food for all the oceanic host. These, and the protozoa which feed upon them, form the basic food supply for the countless myriads of living forms which compose the fauna of modern seas.

Yet, were the conditions of the ocean as they exist to-day to be sought for by some far future geologic delver into the mysteries of the rocks, almost nothing of this profusion of life would be revealed, discovery being nearly or entirely confined to such forms as possess hard skeletons, internal or external, of

which most of these forms are destitute. The same was probably the case with the period which we now have under review, and of whose life we find few forms except those which habitually dwelt upon the bottom. The ocean may have been as full of life then as it is to-day, many of the swimmers of that period, perhaps, representing the ancestral lines out of which the bottom dwellers had evolved, and which are still in a measure preserved for us in modern embryos. These primeval forms may have been even less suitable for fossilization than their counterparts of to-day. The diatoms, the radiolarians, and other minute existing forms have silicious shells capable of preservation. It is quite possible that the early protozoa and protophytes had no such skeletal parts, and that when they died all trace of them departed.

How far back, then, from the earliest age of fossils must we place the actual date of the origin of life? Ages perhaps—epochs—a period as remote from the Cambrian in one direction as we are in the opposite. It may have taken as long, or longer, to develop the trilobite as it since has taken to develop man. During the whole of the immensely long period in which the miles of earlier strata were being deposited, the ocean may have been the seat of an abundant life of the lowest type, and this a very slowly evolving one, the conditions being such that competition and the struggle for existence were not strongly active.

Of the forms of life now existing, the most abundant and the lowest in organization known to us are the bacteria or microbes—omnivorous life specks, feeding alike on animals and plants, and fairly assignable to neither. Possibly life had its origin in forms like these, or in still lower stages of protoplasmic activity, and from this condition developed, after an interminable period, into the simple oceanic protozoa and protophytes typified by the radiolorians and the diatoms, the lowest forms having characters common to both animals and plants, while their descendants divided definitely into plants and animals.

The period here referred to, and that subsequently consumed in the development of the trilobite and its companion forms,

must have been of very great duration; for the conditions were such as to make evolution a slow process. The habitat of these primeval life forms, the oceanic waters, was of the greatest uniformity, even probably in temperature, and possessed no condition likely to provoke rapid variation. There was abundant space and probably abundant food, particularly in view of the minuteness and slight nutritive demands of these early animals, and the struggle for existence could not have been active. Though there were millions devoured hourly, there were trillions provided for the feast, so that no great tendency towards the preservation of favorable variations would have existed.

Yet, though the influences which favor evolution were not very actively present, they could not have been quite absent. The innate tendency to vary which all living forms possess now must have existed then, and the advantage possessed by the more highly over the more lowly organized forms could not have been quite wanting. Consequently, development of varying life forms must have gone on at some rate, and animals must in time have appeared much higher in organization than the simple forms from which they emerged.

And the variations which took place were radical in character. Variation in the higher recent types of life does not penetrate deeply. After ages of change a vertebrate is a vertebrate still. Millions of years of change do not convert a cat into something radically distinct from a cat. But in the primitive period the changes were more profound. Variation went down to the foundation plan of those simple forms and converted them at once into something else. A degree of variation which now would modify the form of a fish's fin may then have converted a monad into a new type of animal. Thus primitive evolution, working on forms destitute of any definite organization, may readily have brought into existence a number of highly different types of life. As the microbe, for instance, may through long variation have given rise to the two organic kingdoms of animals and plants, so the amœba or other low animal form may have varied into the subkingdoms of mollusca, echinodermata, cœlenterata, etc., or rather into simple swimming forms

each of which was the progenitor of one of these great branches of the tree of life.

We are here in a realm of the unknown, through which we are forced to make our way slowly and uncertainly by aid of the clues of embryology, microscopic life conditions, principles of variation and development, and the known conditions of pelagic life. We can only surmise that, as the result of a long era of evolution, the simple primary forms gave rise to a considerable variety of diverse animals, still comparatively minute in size and simple in organization, swimming by means of cilia, and typified to-day by the swimming embryos of invertebrate animals.

As yet—if our hypothesis is well founded—no life existed upon the bottom of the seas, and the swimming forms were destitute of any hard parts capable of fossilization. But why did not some of these forms very early make their way to the bottom and begin life under the new conditions of contact with solid substance? And yet why should they have sought the bottom? Their food supply lay on or near the surface, the bottom of the shallow waters may have been unsuitable through the deposition of soft sediment, and the bottom of the deeper waters very sparse in food. And, more important still, they were quite unadapted to life on the bottom, and needed a radical transformation before they could survive under such conditions. If we look at the remarkable change which the swimming embryo of a star-fish or sea-urchin, for example, goes through before any resemblance to the mature form appears, we may gain some idea of the long series of variations which the primitive ciliated swimmers must have passed through to convert them into crawling or stationary bottom-dwelling forms. Great as was the period needed to produce these type forms of life, another extended period must have been necessary to convert them into well adapted habitants of the solid floor of the seas.

(To be Continued.)

BIRDS OF NEW GUINEA (FLY CATCHERS AND OTHERS).

BY G. S. MEAD.

Among the many kinds of Flycatchers (*Muscicapidæ*) inhabiting the Papuan Islands, while there is dissimilarity in so large a number of species, yet there are not those striking differences amounting almost to contrasts which characterize birds of greater size. Many species have been unnoticed by travellers and other writers; many exist only in cabinets and collections, labelled and ticketed, or at most given a few lines of technical summarization in catalogues. With the rank and file of birds anything more than this is impossible. Sometimes a particularly attractive specimen of *Malurus* or *Rhipidura* or *Pratincola* calls attention to itself, or mere accident brings an individual to the notice of the explorer or student.

Thus Mr. Wallace notes pointedly "the abnormal red and black flycatcher," *Peltops blainvillii*, so named by Lesson and Garnot many years since. It is a sprightly, highly colored bird with the predominant hues strongly contrasted and still further accentuated by spots of white on the head and beneath the wings. In flight this active little flycatcher presents in turn these conspicuous markings with striking effect. The red tint is a bright crimson spread over the lower back and tail coverts. The main color is a steely-green black covering with greater or less intensity the seven inches of total length. The genus is represented by this species only.

The same notable expedition to South Eastern New Guinea that secured the two beautiful prizes *Cnemophilus macgregorii* and *Amblyornis musgravianus*, discovered also a new species of flycatcher, viz., *Rhipidura auricularis.* It is described as having the "upper surface smoky gray; head brownish black; tail the same above and below; bill dark brown; legs black." The head is marked by black and white stripes, found upon the wings as well. Upon the chin, throat and breast similar

lines of unequal width are plainly drawn. The under parts are in general buff varied with black and gray. Dots and bars of white appear on the wings and tail. Its total length is about six inches.

Rhipidura leucothorax, the Whitebreasted Fantailed Flycatcher, is much more widely distributed, being met with in different parts of New Guinea. The descriptive name here describes very imperfectly, for the breast is by no means entirely white as might be inferred; black is almost as prominent, alternating with the white which shows in spaces, though lower down it crowds the black into narrow bands or crescents. The general color of the bird above is brown, becoming dark upon the head, still darker over the bill. The wings are black, finished off with white spots. This is the appearance too of the tail feathers as well as of the under side of the wings. There are also white streaks and lines about the sides of the head and throat. Bill black above. Length 8 inches.

The family of Wood Songsters (*Pœcilodryas*) are all small birds rarely exceeding 6.5 inches in total length. The coloration is in general black and white, the former greatly predominating. *Pœcilodryas albinotata* at first sight looks in color not unlike those fine drongos, the Edolias. In this instance, however, leaving the disparity of size out of account, the gray is not nearly so uniform, a dull black and a deep black appearing on the wings, tail and throat including the side face. A patch of white meets the black on the sides of the neck. White again is seen on the abdomen and under tail coverts, becoming discolored along the flanks and sides of the body.

Pœcilodryas papuana comes from the same region of the Arfak Mountains as the foregoing species. It is considerably smaller in size measuring only 4–5 inches inches in length, but of brighter color. This is a yellow, somewhat dull an becoming light brown on the wings and tail. Head and neck are darker than the body. A crescent of orange runs from the bill over the eye.

Pœcilodryas leucops shares the same habitat. It is not unlike the preceding in coloration of the body but that of the head, nape and throat is entirely different. In this case it is

a dark gray, to gray on the neck with darker feathers over the eye. White marks the upper throat and chin and appears as a prominent spot in front of the eye. Total length nearly five inches.

From the Arfak Mountains also comes *Pœcilodryas bimaculata* and from the same general region *Pœcilodryas hypoleuca* and *P. brachyura* and *P. cinerea.* The first is conspicuously black and white, the former color preponderating very largely of course, while the white shows as bands and bars or stripes. It is most apparent on the lower parts where it may be reckoned as the ground color.

P. hypoleuca, the Whitebellied, is a rather larger bird, reaching the length of 6 inches. The general color is dusky above, relieved by white patches on the head. The same color covers the under parts set off by black on breast and throat. The last named—*P. brachyura,* the Shorttailed—is marked similarly with the tones rather deeper and clearer. Length 5.5 inches.

Monachella mulleriana or *saxicolina,* a Chatlike Flycatcher, is a lively little bird found as well in the south of New Guinea along the Fly River, as in the north among the Arfak Mountains. It is of grayish plumage above becoming nearly white on the rump and tail coverts; tail feathers and wings are dark brown. The head is also dark brown with a line of white over the eye. A spot of black lies near the bill. Below the colors are nearly those of the upper parts, that is, the body is a soft white, the wings brown. Bill and feet black. The sexes are alike in markings and size, the length being about six inches. They are both assiduous in the pursuit of insects, generally along streams on level spaces.

Monarcha or *Muscipeta melanopsis,* the Carinated Gray Flycatcher, has a ring of short black feathers about the large full eye, a discriminating characteristic, imparting with the strong prominent bill a singular appearance to this Australian bird. The entire throat and part of the face are also black, crowded upon by the soft slate color which becomes deeper over the rest of the body. The long tail above is dusky; below, as well as under the wings and on the abdomen, the color is a

bright rufous. The female is unmasked about the head and throat. Feet plumbeous. Length 7 inches.

One of the loveliest, certainly the most brilliant of Flycatchers is *Monarcha chrysomela*, the Goldenhooded. Blue-black and gold are the boldly contrasted colors of this bright little creature whose length is 6 inches. The ground color is orangeyellow; this is almost equally rich whenever it is spread. Jet black with a blue gloss covers the entire throat and upper breast, the upper back, the outer wing feathers and tail. An irregular stripe of the same bends round the shoulder. The deepest black is on the throat where the thick plumage is metallic. The crown is roughened into a kind of crest. The bill and feet are black. All besides, as has been said, is a lovely yellow, making the bird a most conspicuous object among the dark trees.

Todopsis cyanocephala of the *Muscicapidæ* is adorned with a blue crown, as its name indicates. This rich color appears besides on the neck, back and wings though of a somewhat different shade. A purpleblack runs down the lower back and covers the tail, excepting the two middle feathers which are of bluish tinge. The under parts are of a dark purple also, becoming black beneath the wings. The bill and feet are dull black. The length of the male bird is rather more than six inches, the female about an inch less. Her coloring is almost as rich, but different. A warm brown takes the place of black above, a light buff of the black below, though along the sides as far as the under tail coverts the brown reappears. Blue colors the head and stripes the neck, showing lighter on the tail where it is much mingled with white.

Malurus albiscapulatus is scarcely 4 inches in length but is not only of rich velvety plumage but of conspicuous appearance also, for its white patches on a black ground color attract attention at once. These patches occur on either side of the body both above and below, those above showing finely when the bird is in flight, those below lining the chest from the bend of the wings. Elsewhere the plumage is a deep black of a bluish cast, soft and lustrous. The home of the species is in Southeast as well as Northwest New Guinea.

Caterpillarcatchers (*Campephaga*) abound in New Guinea of varying degrees of beauty, some being bright of hue, others almost somber. A few individuals not in strict order are considered here.

Campephaga sloetii or *aurulenta*, according to d'Albertis (Vide Journal), is a rare bird in collections but is distributed all over New Guinea. He found it most numerous far up the Fly River, but obtained but one specimen in a native's garden, feeding on the small berries of a tall tree. It is a yellow bird, very vivid on certain parts, duller on the wings where there is more or less black and white as well, and golden yellow on the breast and abdomen. The head, sides of head and throat are marked with gray, black greenglossed, and a band of white. White inclining to yellow lines the under wings. Bill, feet and eyes are black. The bill is short and strong.

Where the male bird is brilliant and positive in color, the female assumes paler shades and neutral tones. She is somewhat longer, measuring nearly 8 inches in total length.

The tail feathers of the male are marked with white, especially the outer ones.

The mountain Cuckoo-shrike, *Campephaga montana* or *Edoliisoma montana* is a fine bird from the Arfak region. The contrasted colors, bluegray above, black below, are so carefully marked as to render their wearer easily distinguished from his kind. The same may be said of the female who is equally conspicuous in unusually clear colors and a perfectly black tail.

The Bluegray Campephaga, *Campephaga strenua* (Schl.), from about the same region is colored mainly as its name indicates, the customary black appearing on the throat and in a line on the head. The bill and feet are also black; some of the tail feathers likewise, but a rusty tinge marks the lower wing coverts. The bill is unuusally powerful for so small a bird.

Campephaga melas or *Edoliisoma nigrum* is found in different parts of Papua. It is a larger bird and with a coloration not at all characteristic of the class to which it bears so similar a name. The male is of a glossy black, reflecting purple along

the wing and tail coverts. The female, longer than her mate in size, is also quite distinct in plumage. A marked reddish dye takes the place of lustrous black. On the head the color is warmer than on the body. The wings are shaded, in some individuals dusky.

Edoliisoma tenuirostris or *Campephaga jardinii* (Gould), the Slenderbilled Cuckoo-shrike, is an Australian bird but found also in New Guinea near Port Moresby. It is about a foot long, of a cloudy blue color, excepting on the side face where it becomes black, and on wings and tail which contain rather more black than blue. The outer tail feathers underneath terminate in white. The bill is black and anything but slender. Feet black, eyes brown.

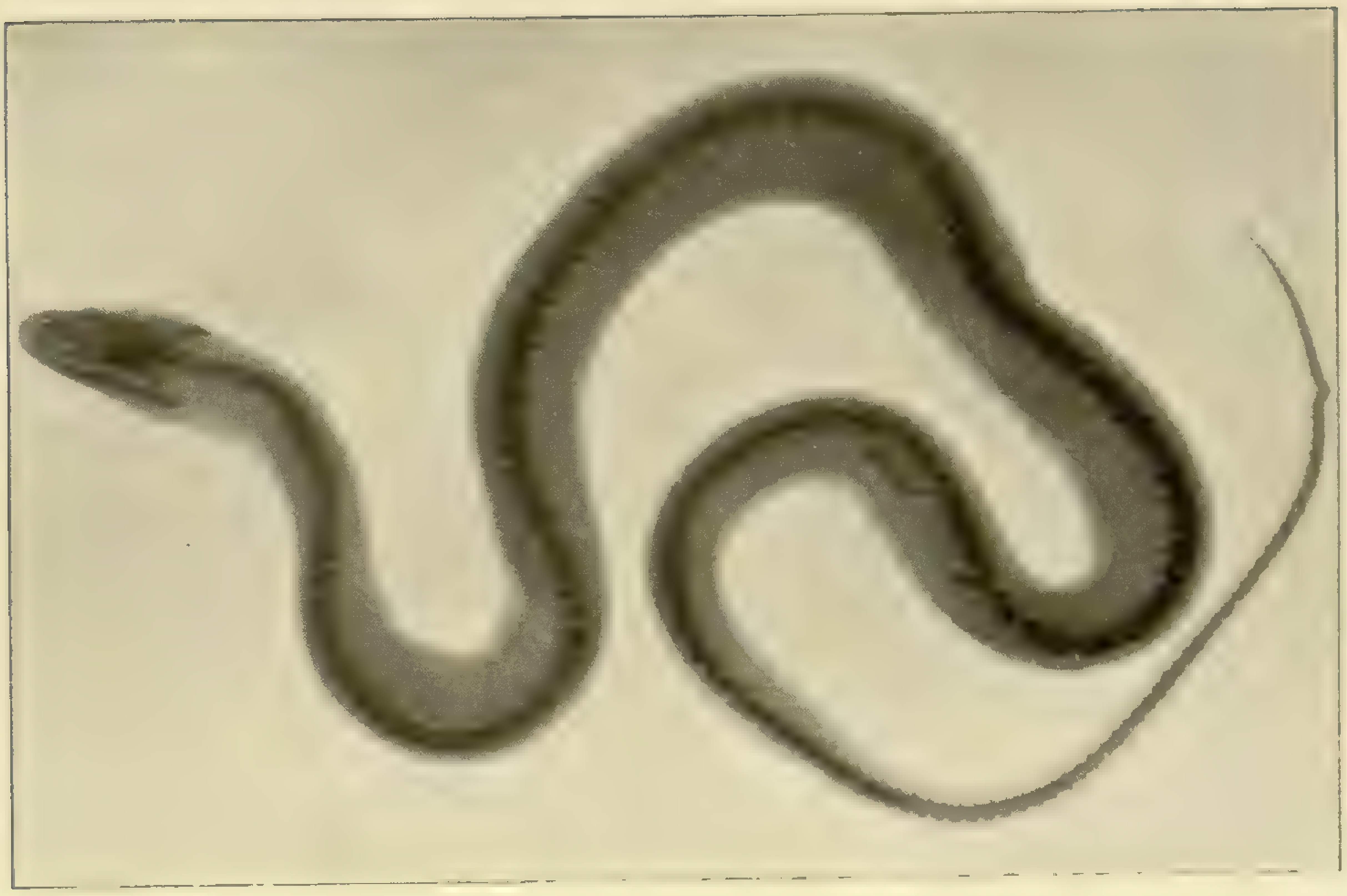
In 1882 the nest was found by Mr. C. C. L. Talbot in a Eucalyptus tree. It was composed of wiry grasses securely fastened together with cobwebs on the thin forked horizontal branch. The eggs laid in the small shallow depression were ovoid in shape and of a pale bluish gray ground dotted irregularly over with dark brown spots and lines.

(*To be Continued.*)

EDITOR'S TABLE.

—We notice that the project of a National University to be established at Washington has been again brought up before Congress. Washington has many advantages as a location for a university, and the Methodists and Catholics have not been slow to take advantage of them. The Columbian University, a non-sectarian institution, is located there. That it devolves on the nation under any circumstances to establish a university there or anywhere else we fail to perceive. So long as institutions of this kind exist either by virtue of State support or private munificence, there is no necessity for the intervention of the Government in this part of the educational field, but there are strong reasons why it should not do so. The financial basis of all institutions supported by congressional appropriations is always precarious. The subsidies are liberal while they last, but changes in the fiscal policy of the Government produce fluctuations in the revenue, and expenditures are varied accordingly. Then the faculty of such an institution would be under bonds to please the congressional majority,

PLATE V.



1

2

1. *Natrix compressicauda.* 2. *Lepomis sp.*

or the revenues might be reduced or suspended. The teachings on certain subjects might be interfered with or controlled by the appointing power, and the appointments to positions would probably become political perquisites. Nothing more disastrous to the proper conduct of a university can be imagined, and an institution established under such conditions would soon cease to be a credit to the nation. We hope that the project will not prevail, not only for these reasons but for another. This is, that the Government has in connection with its departments various commissions and bureaus, which occupy themselves with original scientific research in connection with the various economic objects of their care. These should be continued and expanded if possible, and not, as is sometimes the case, weakened by insufficient appropriations. If the Government at Washington will support this work it will be doing more for education than any university can do, and will continue to add to its credit among nations in the future as it has done in the past.

—The X-rays of Roentgen will prove of some utility to some branches of biological research by disclosing the characters of mineral substances enclosed within the walls of animals and plants. A good many characters of the skeleton, for instance, may be detected in specimens which cannot be spared for maceration, and other applications will occur to both botanists and zoologists. We present, as an illustration, a sciagraph of a species of sunfish (Lepomis), made by Messrs Leeds and Stokes, of Queen & Co., of Philadelphia.

—Another excellent journal, this time a French one, has been led astray by attaching too much importance to the romances of the American newspaper reporter. We refer to the story published some months ago by a San Francisco journal that a physician of that city had succeeded in grafting some snakes together by their tails. The fictitious character of the narrative is demonstrated by the statement that the said physician selected snakes in which the vertebral column does not extend to the end of the tail. If the editor of the journal had referred the question to the professors of the Museum of Paris, he would have learned that snakes of this kind exist only in the imagination of the author of the canard.

—We published a statement some months ago that Mr. L. O. Howard of U. S. Dept. of Agriculture had discovered that the application of oil to water where mosquitoes breed, destroys both the eggs and the larvæ of those pestilent insects. We are reminded by an exchange that the alleged discovery was made by Mrs. Eugene Aaron in Phila-

delphia. We were probably indiscreet in referring to Prof. Howard's observations as involving more than a modicum of "discovery." On examination we find that the knowledge of this mode of destroying mosquitoes antedates not only his observations, but also those of Mrs. Aaron. The information has, however, not been generally disseminated until recently.

—The American Society of Naturalists, at its last meeting, adopted a resolution commending to the public the importance of Antarctic exploration. A committee of three was appointed to take measures looking towards sending an expedition to Antarctica in the near future. At about the same time England and Australia joined in supplying the funds necessary for such an exploration of the land lying south of Tasmania within the Antarctic circle. The natural object of an American expedition is, of course, the exploration of Graham's Land, which lies due south of Patagonia. For the advance of knowledge of the physics of the globe, explorations of the polar regions are of the first importance; and the results to the history of its biology in past ages, will be scarcely less important. America has done her full share of Arctic exploration; and in the person of Commodore Wilkes made a beginning in Antarctic work. It is now fully time for us to resume this work, and it is to be hoped that the means of sending the expedition may be speedily obtained.

—The Huxley Memorial Committee have raised the considerable sum of £1532, and are considering the uses to which it may be put. It has been resolved to erect a statue of Huxley in the British Museum, and to endow the award of a medal for meritorious work in biology. It is now desired that the amount may be increased for the purpose of creating another endowment. Should sufficient subscriptions be obtained in America, it might become appropriate that this new endowment should have its seat in this country. The scientific men of America hold in high esteem the biological work of Huxley, and there are many reasons why a foundation in his memory would be grateful to Americans.

RECENT LITERATURE.

Williams's Manual of Lithology[1] is written for the "beginner in the subject who wishes a thorough knowledge in the presentation of the subject, in a fuller and more compact arrangement than can be obtained in geological text-books. The arrangement is such that those who wish to continue the work in the microscopic analysis of rock forming minerals, as taught in petrography, will have nothing to unlearn."

The latter statement of the author is not quite true, for, in his classification of the rocks discussed, he places among the crystalline schists quartzite, pyroxene rock and olivine rock that present no traces of foliation. In the main, however, the classification is good. The rocks are divided into Primary Rocks and Secondary Rocks, and each of these groups is separated into "Divisions" in accordance with their chemical composition. Of the different families or "divisions" the effusive rocks are first described and then the intrusive ones. The Secondary Rocks embrace the Débris, the Sedimentary and the Metamorphic divisions, the first of which differs from the second in consisting of unconsolidated materials.

Nearly all the rock varieties recognized by petrographers are briefly described, and even many that are no longer recognized as distinct types. The descriptions are all based on macroscopic characters, but they are, in most cases, full enough to enable the user of the book to identify the type.

The terminology made use of in the description is somewhat different from that used in petrographical text-books, but, since it is employed in the description of hand specimens and not of their sections, this is to be expected. All the terms used are clearly defined, and many of the new ones introduced are perhaps needed.

The main faults to be found with the volume are that it attempts to discriminate between too many rock types, and that it contains too many rock names that have long since gone out of use. In spite of these faults, the treatise is a valuable one, and it should meet with success. The typographical work is excellent. The plates are from photographs, and are illustrative of rock structures.—W. S. B.

[1] Manual of Lethology: Treating of the Principles of the Science, with Special Reference to Meguscopic Analysis. By Edward H. Williams. 2d Ed. New York: John Wiley & Sons, 1895. Pp. vi, 418; plates 6. Price, $3.00.

The Corundum Deposits of Georgia.[2]—This preliminary report on the corundum deposits of Georgia, by Francis P. King, has been issued as Bulletin No. 2 by the Geol. Survey of that State. The importance of corundum in the arts, and the high price paid for it, together with the fact that Georgia ranks second in the Union in the production of this mineral, make the report of special interest. The introductory chapters give the history, varieties and associate minerals of corundum, succeeded by a brief account of the geology of the crystalline belt in which the mineral occurs and the distribution of deposits. Several pages are given to the economics, including natural and artificial abrasives. There is also a bibliography of the American literature upon the subject.

The map accompanying the report is well-colored, showing at a glance the different formations. The other illustrations are reproductions from photographs, showing out-crops of the mineral-bearing veins.

Bailey's Plant Breeding.[3]—No man in the country, perhaps, is better prepared to write a book on plant breeding than the accomplished professor of horticulture in Cornell University, and it is a pleasure to find that in the preparation of the work before us he has not disappointed his friends. There is, as the author says in his preface, much misapprehension and imperfect knowledge as to the origination of new forms of plants, and much of what has been written on the subject is misleading. "Horticulturists commonly look upon each novelty as an isolated fact, whilst we ought to regard each one as but an expression of some law of the variation of plants." The author might have included in the foregoing many "botanists" as well as the horticulturists, for, unfortunately, it is true that many who call themselves botanists, and who hold positions in honored institutions, have not yet risen to a biological conception of the science which they profess to cultivate.

Among the topics treated in these lectures are the following, viz.: individuality, fortuitous variation, sex as a factor in the variation of plants, physical environment and variation, struggle for life, division of labor, crossing, etc.

The book should be in every botanist's library, and every teacher of botany will do well to make copious extracts from it in his lectures.

[2] A Preliminary Report on the Corundum Deposits of Georgia. By Francis P. King, Bull. No. 2, Georgia Geological Survey, Atlanta, 1894.

[3] *Plant Breeding*, being five lectures upon the Amelioration of Domestic Plants. By L. H. Bailey. New York: Macmillan & Co., 1895. pp. xii, 293, 12 mo.

The following, from page 135, will show many teachers that much may be learned from this book: "Some two or three years ago, a leading eastern seedsman conceived of a new form of bean-pod which would at once commend itself to his customers. He was so well convinced of the merits of this prospective variety, that he made a descriptive and "taking" name for it. He then wrote to a noted bean-raiser, describing the proposed variety and giving the name. 'Can you make it for me?' he asked. 'Yes, I will make you the bean,' replied the grower. The seedsman then announced in his catalogue that he would soon introduce a new bean, and, in order to hold the name, he published it along with the announcement. Two years later I visited the bean-grower. 'Did you get that bean?' I asked. 'Yes; here it is.' Sure enough, he had it, and it answered the requirements very well."

—CHARLES E. BESSEY.

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General Notes.

PETROGRAPHY.[1]

The Eruptives of Missouri.—Haworth[2] has described in much detail the dykes and acid eruptives in the Pilot Knob region, Missouri. The dyke rocks are typical diabases, diabase-porphyrites, quartz-diabase-

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Mo. Geol. Survey, Vol. VIII, 1895, p. 83-222.

porphyrites and melaphyres. The author unfortunately classes as diabase-porphyrites both glassy and holocrystalline rocks. The acid rocks of the region include granites, granite-porphyries, porphyrites and quartz-porphyries. The first two are characteristically granophyric. Their orthoclases are often enlarged by granophyre material whose feldspar is fresh, while the nucleal feldspar is much altered. The quartzes likewise, are enlarged by the addition of quartz around them. There were two periods of crystallization in these rocks. In the second period the phenocrysts were corroded and the groundmass was produced. In addition to the quartz and orthoclase there are present in these rocks also biotite, hornblende, plagioclase and a number of accessory and secondary components. The porphyries and porphyrites contain the same constituents as the granites, from which they are separated simply on account of differences in structure. The phenocrysts are mainly orthoclose, plagioclase, microcline and quartz, many of which are fractured in consequence of magma motions. The groundmass in which these lie is of the usual components of porphyry groundmasses, and in texture is micrograhitic, granophyric, micropegmatitic and spherulitic. Many of the porphyries contain fragments of their material surrounded by a matrix of the same composition in which flowage lines are well exhibited. These rocks are evidently volcanic breccias. The author divides the porphyritic rocks into porphyries and porphyrites, the latter containing plagioclase phenocrysts and the former phenocrysts of quartz, orthoclase and microcline.

Rocks from Eastern Africa.—The volcanic rocks of Shoa and the neighborhood of the Gulf of Aden in Eastern Africa comprise a number of varieties that have been carefully studied by Tenne.[3] The main mass of the mountains of the region consists of biotite-muscovite gneiss. This is cut by nepheline basanites, the freshest specimens of which contain phenocrysts of olivine, augite and feldspar in a groundmass of plagioclase, augite, nepheline and often olivine. Trachytes, phonolites and basalts occur in the Peninsula of Aden. The trachytes include fragments of augite-andesite. Inland granophyres with pseudospherulites in their groundmass, trachytes and feldspathic basalts were met with. The granophyres are much altered. In the fine grained product formed by the decomposition of the groundmass of one occurrence quartz, feldspar, and a blue hornblende with the properties of glaucophane can be detected. All the rocks are briefly described. They present no peculiar features other than those indicated.

[3] Zeits. d. deutsch. geol. Ges., XLV, p. 451.

A Basic Rock derived from Granite.—Associated with the ores in the hematite mines of Jefferson and St. Lawrence Counties, N. Y., is a dark eruptive rock that was called serpentine by Emmons. Smyth[4] (C. H.) has examined it microscopically and has discovered that it consists of a chlorite-like mineral, fragments of quartz and feldspar. By searching carefully he discovered less altered phases of the rock that were identified as granite. The peculiar alteration of an acid granite to a basic chlorite rock is ascribed to chemical agencies. According to the author's notion the pyrite in a neighboring highly pyritiferous gneiss was decomposed, yielding iron sulphates and sulphuric acid. These solutions passed into limestone yielding the ores and then into the granite changing it into chlorite. The altered rock is found only with the ores. The original was probably not always granite. An analysis of the altered rock gave:

SiO_2	Al_2O_3	Fe_2O_3 FeO	MgO	CaO	Na_2O	K_2O	H_2O	Total
29.70	17.03	27.15	10.66	1.68	.56	.10	11.79	= 98.67

Cancrinite-Syenite from Finland.—In the southeastern portion of the Parish Kuolajaroe in Finland, Ramsay and Nyholm[5] secured specimens of a nepheline-syenite containing a large quantity of what the authors regard as original cancrinite. The rock is found associated with gneissoid granite at Pyhakurn. The rock is trachytic in structure and is composed of orthoclase, aegerine, cancrinite and nepheline as essential constituents and apatite, sphene and pyrite as accessories. The cancrinite was the last mineral to crystallize. It occupies the spaces between the other components, and yet it often possesses well defined hexagonal forms. It occurs also as little prisms included within the orthoclase. Because of this association and because the nepheline in the rock is perfectly fresh the cancrinite is regarded as original. This mineral comprises 29.04% of the entire rock.

The same authors in the same paper describe a porphyritic melilite rock found as a loose block a few kilometers W. N.-W. of Lake Wuorijarvi. It contains large porphyritic crystals of melilite, pyroxene and biotite in a groundmass composed of labradorite, zeolites and calcite. The pyroxenes are made up of a colorless augite nucleus surrounded by zones of light green aegerine-augite and deep green aegerine. No olivine was detected in any of the thin sections.

[4] Jour. Geology, Vol. 2, p. 667.

[5] Bull. Com. Geol. d. l. Finn., No. 1.

Rocks from the Sweet Grass Hills, Montana.—Weed and Pirsson[6] describe the rocks of the Sweet Grass Hills of Montana as quartz-diorite-porphyrites, quartz-syenite-porphyries and minettes. The first named rock presents no special peculiarities. The quartz-syenite-porphyry contains orthoclase, plagioclase and augite-phenocrysts in a fine groundmass of allotriomorphic feldspar and quartz. The augite is in short thick prisms composed of a pale green diopside core, which passes into a bright green aegerite mantle. The minette also contains aegerine, but otherwise it is typical.

Petrographical News.—Two peculiar phonolitic rocks are described by Pirsson[7] from near Fort Claggett, Montana. One is a leucite-sodalite-tinguaite, with leucite pseudomorphs, and sodalite as phenocrysts in a groundmass composed mainly of a felt of orthoclase and aegerine. The leucite pseudomorphs are now an aggregate of orthoclase and nepheline. In the centers of some of them are small stout prisms of an unknown brown mineral, that is pleochroic in brownish and yellowish tints. The second rock is a quartz-tinguaite porphyry somewhat similar to Brögger's grorudite.[8]

In a few notes on the surface lava flows associated with the Unkar beds of the Grand Cañon series in the Cañon of the Colorado, Ariz., Iddings[9] briefly describes compact and amygdaloidal basalts and fresh looking dolerites that are identical in all respects with modern rocks of the same character.

Laspeyres[10] estimates that the quantity of carbon-dioxide in liquid and gaseous form contained in rocks is sufficient to serve as the source for all that which escapes from the earth's natural fissures as gas, as well as that which escapes in solution with spring water. It may be set loose from the rocks through the action of heat or through the action of dynamic forces.

In a handsomely illustrated brochure Merrill[11] describes the characteristics of the onyx marbles and the processes by which they originate. Differences in temperature, according to the author, are not the controlling conditions determining the differences in texture between the onyxes and travertine. He is inclined to the belief that the banded onyxes were formed by deposition from warm solutions under pressure flowing into pools of quiet cold water.

[6] Amer. Jour. Sci., Vol. I, p. 309.

[7] Amer. Journ. Sci., 1895, Nov. p. 394.

[8] AMERICAN NATURALIST, 1895, p. 567.

[9] 14th Ann. Rep. U. S. Geol. Survey, p. 520.

[10] Korrespond. bl. Naturh. Ver. preuss. Rheinl., No. 2, 1894, p. 17.

[11] Rep. U. S. Nat. Mus., 1893, p, 539.

In a preliminary report on the Geology of Essex County, N. Y., Kemp[12] describes the occurrences of the gneisses, limestones, ophicalcites, gabbros, lamprophyres and other igneous rocks of the district, and gives an account of their geological relationships.

GEOLOGY AND PALEONTOLOGY.

Bear River Formation.—The explorations of Mr. Stanton and Mr. Charles White in the Bear River Valley have been the means of correcting a long standing error among geologists concerning the taxonomic position of strata known as the Bear River Formation. A summary of the facts as presented by Mr. White in a late Bulletin of the U. S. Geol. Survey shows that the formation under discussion is not Laramie, to which age it has been hitherto been referred, but belongs to the Upper Cretaceous, at or near the base of that series. That is its position has been determined by Mr. Stanton as beneath the Colorado formation, and above that series of Jurassic strata which occurs within a large part of the interior region of North America generally regarded as of Upper Jurassic age and which in the general section given is called "Dakota?" This accords with the reputed age of a formation in Hungary, whose fauna is more nearly like that of the Bear River series of strata than of any other known.

Mr. White, therefore, defines the Bear River series as a distinct formation stratigraphically, geographically, and paleontologically, and states in detail its taxonomic position. All the known fossils of the formation are described and figured, comparisons are made of its fauna with those of other nonmarine formations of this and other continents, and relevant biological questions are discussed.

In making a general comparison of the Bear River fauna with the other nonmarine fossil faunas of North America, Dr. White calls attention to those features of the Bear Fauna by which it differs conspicuously from all the others. Reference is here especially made to the Auriculidæ and Melaniidæ, because it is members of these two families that give the Bear River Fauna its most distinctive character. In this connection the author remarks "this faunal character is all the more conspicuous because, of the six genera which represent those two families, only two of them are known in any other North American fauna, either fossil or recent."

[12] Report of State Geologist [of New York] for 1893, p. 433.

The similarities and contrasts between the fauna of the Bear River formation and those of the other nonmarine beds of North America leads to a discussion of their causes. The author suggests that certain genetic lines of descent have become diverged from the main lines of succession and destroyed by some of those physical changes which mark successive epochs, and adds "we may reasonably assume that one of those divergent lines terminated in the Bear River fauna; that is, at the close of the Bear River epoch the area which its nonmarine waters had occupied having become overspread by the marine waters in which the Colorado formation was deposited, it is not probable that any fluvial outlet of the former nonmarine waters was perpetuated, and there was, therefore, no provisional habitat in which the Bear River fauna might have been preserved. It was probably in this way that the distinguishing types of that fauna became extinct, together with others of its members which were not so specially characteristic of it." (Bull. U. S. Geol. Surv. 128, Washington, 1895.)

On the Occurrence of Neocene Marine Diatomaceæ near New York.—The rocks which contain Diatomaceæ (or Bacillariaceæ) in America are clayey, that is to say they contain more or less of clay, and they vary in color from a nearly white to a fawn color and to a greenish, greyish-brownish or almost black. They are not older than the Oligocene nor newer than the Plistocene. They can be placed in the Neocene, a period that ranges from the Eocene to the Plistocene, and not in the recent. Those I have to describe in New York are not Miocene, but they belong to a place which may provisionally be classed as Pliocene or Plistocene of the European geologists.

Ever since 1843, the so-called infusorial earth has been known in Virginia and was thought by Rogers the discoverer to be Miocene Tertiary, he classifying it as the European rocks were. Bailey accepted the classification and so did the later geologists. When fresh water fossil Diatomaceæ were found in Massachusetts they were thought to be Miocene also without studying the rocks themselves and seeing how they stood in the geological scale. When they were found in New Hampshire I did not classify them nor did Hitchcock attempt to do so. They were placed in the lacustrine Sedimentary and provisionally in the Recent. But now they can be seen to be older than the Recent and must be placed in a position by themselves. In the Iceberg period, the Champlain, when the ice which covered the country was beginning

to melt, icebergs which formed by the breaking off of the ice on the border were common. The icy water had Bacillariaceæ in it, for they existed, as they do now, when the temperature was at 0° C. This flowed down to the lower regions from the north and northwest.

In California I did not classify the rocks containing the Bacillariaceæ leaving that to the older and more experienced geologists. Blake, who had discovered them at Monterey, supposed them to be Miocene, for he saw as Bailey showed them to be similar to the Virginian ones. In Japan where I discovered them also I failed to classify them for Pumpelly, who had brought them home did not place them likewise. When the infusorial earth was found in Florida, it had also been placed in the Miocene Tertiary by Bailey. And when I had it from that state subsequently at Manatee, I failed to classify it because I had not visited the spot where it came from myself. Now I believe these are older than what is called the Miocene. And I am confirmed in this supposition by what Towney said of the Virginia stratum. I prefer to place them as far back as the Upper Eocene, the Oligocene as it is called. In New Jersey at Asbury Park and Atlantic City the infusorial earth has been found by Woolman and classified by him as Miocene. But further north on the Atlantic side of the continent it has not been seen. I examined the clay that was dug at about two feet down at Foley's, South Beach, Staten Island, N. Y., but although it contained marine Bacillariaceæ it was not what I wanted. I thought it belonged to the Raised Coast period. At Martha's Vineyard, Mass. the clay classed as Miocene by Dall did not contain any Bacillariacæ.

It was on the 11th of August, 1895, that I visited Rockaway to get rest from the turmoil and heat of the city. Rockaway is a beach or promontory which extends down from a place called Far Rockaway southwards on the coast of Long Island. Long Island is made up of hills of no great height extending down the middle or on the north shore of the island. A low range of country extends down the southern shore where the Atlantic Ocean begins. It is fringed by sandy bars which are mostly islands. These islands extend down the coast from Cape Cod, Mass. to Florida. Key West is the most southern of the islands which are known in Florida as Keys. The country on the Atlantic side of the island is low, sloping down to the coast without any elevation in it.

I knew that I should go down by rail cutting through the hills until I came transversely to the island to the promontory of Rockaway. It is true that I wanted to get out of the cities heat but I had also two other reasons for going. I wanted to study the glacial phenomena which I

knew would present themselves there. At the same time I desired to search for the infusorial earth. At one place we came to a kettle hole, at the Lutheran Cemetery. I was sure it was a kettle hole and knew there was clay, a Lacustrine Sedimentary deposit of Diatomaceæ, at the bottom. I saw the glacial moraine made up of gravel and sand all along the road. The moraine was a gravelly till with boulders scattered through it. On the top it was capped by a layer of about three feet thick of whitish clay. This I knew to be diatomaceous, the same as covers the country in New Jersey and on Manhattan or New York Island. As we approached the station known as Brooklyn Hills we cut through three high hills which I saw then and afterwards were made up of moraine stuff, mostly gravel, with a white clay about three feet thick on top. The clay was the same as we had just passed. It makes the bottom of the glacial clay, the Lacustrine Sedimentary deposits of Diatomaceæ. In this moraine I afterwards got a small distinctly striated boulder and near the bottom of the hill, about twelve feet from the bottom was a grey clay with Hematite nodules in it. Cretaceous clay no doubt.

The country became flat with no rising in it and sloping gradually towards the coast where we came to the station known as Aqueduct. Cretaceous clay underlies the country doubtless covered by glacial till or moraine. At Aqueduct the railroad runs out on tressels to Rockaway. At Rockaway Beach I landed and wandered south on the promontory but found nothing but white siliceous sand, they were not digging anywhere that I could find. I wandered north in the direction of Far Rockaway where the land became higher and was covered by the whitish Iceberg period clay which evidently came from the north-west. At Auvergne they had been digging a ditch to reclaim the land from the sea. This was on the opposite side of Rockaway to the Atlantic Ocean, on Jamaica Bay. The digging was over six feet deep. They had thrown out some of the Iceberg clay and below that some greyish soil without any stones in it. I saw at once that it was different in character from the soil on the marshes and which I had learned belonged to the Raised Coast or Champlain Period. I took some home and examined it and came to the conclusion that I had found what I was in search of, the infusorial earth. It was no doubt what may be termed Pliocene Tertiary and belonged to the Neocene Period.

I cleaned some and found the following Bacillariceæ in it besides some forms of *Dictyota*, which are Radiolaria. So me few usual forms escaped me but will probably be found hereafter.

Achnanthes subsessilis C. G. E.
Actinocyclus ehrenbergii J. R.
Actinoptychus undulatus C. G. E.
Auliscus cœlatus J. W. B.
Auliscus pruinosus J. W. B.
Auliscus radiatus J. W. B.
Aulacodiscus germanicus C. G. E.
Amphora ovalis F. T. K.
Amphiprora elegans W. S.
Amphiprora navicularis C. G. E.
Amphiprora pulchra J. W. B.
Biddulphia aurita A. B.
Biddulphia pulchella G.
Biddulphia rhombus W. S.
Cerataulus radiatus J. R.
Cerataulus smithii W. S.
Cerataulus turgida W. S.
Coscinodiscus asteromphalus C. G. E.
Coscinodiscus excentricus C. G. E.
Coscinodiscus subtilis C. G. E.
Coscinodiscus lineatus C. G. E.
Coscinodiscus nitidus W. G.
Cocconeis scutellum C. G. E.
Cyclotella striata F. T. K.
Dicladia mitra J. W. B.
Doryphora amphiceros F. T. K.
Epithemia turgida F. T. K.
Epithemia musculus F. T. K.
Eunotia monodon C. G. E.
Euotiogramma amphioxys C. G. E.
Fragillaria pacifica A. G.
Grammatophora marina F. T. K.
Hyalodiscus franklinii C. G. E.
Hyalodiscus stelliger J. W. B.
Isthmia enervis C. G. E.
Melosira sulcata C. G. E.
Navicula clavata A. G.
Navicula didyma C. G. E.
Navicula elliptica F. T. K.
Navicula hennedii W. S.
Navicula humerosa A. B.
Navicula lacustris W. S.
Navicula lata A. B.
Navicula peregrina F. T. K.
Navicula permagna J. W. B.
Navicula viridis C. G. E.
Nitzschia acuminata W. S.
Nitzschia balanotis A. G.
Nitzschia sigma F. T. K.
Nitzschia tryblionella H.
Plagiogramma gregoriana R. K. G.
Pleurosigma angulata W. S.
Pleurosigma balticum C. G. E.
Pyxilla ? baltica A. G.
Pyxidicula compressa J. W. B.
Rhabdonema arcuatum F. T. K.
Roicosphenia currata F. T. K.
Scoliopleura tumida L. R.
Schizonema fœtida J. E. S.
Stauroneis aspera C. G. E.
Stauroneis birostris C. G. E.
Stephanopyxis appendiculata C. G. E.
Stephanopyxis turris J. R.
Surirella febigeris F. W. L.
Surirella striatula B. V.
Synedra affinis F. T. K.
Terpsinoe americana J. W. B.
Triceratium alternans J. W. B.
Triceratium favus C. G. E.
Triceratium maculatum F. T. K.
Triceratium punctatum T. B.

These are all the Bacillariaceæ that I have detected up to this time. There are several forms of *Dictyocha*, a genus of Radiolaria, present

also. And what I consider a new genus of Bacillariaceæ, which I have called *Ancile radiata.* It is free and found rarely in the salt water in Jamaica Bay, Rockaway and at Foleys, and South Beach, Staten Island. But of this I shall speak hereafter. Mr. W. A. Terry says he has found broken fragments of *Brunia* but this I myself have not seen, although common in a deposit which I will also describe hereafter taken at fifteen feet from the surface at Hoboken, N. J, I, another day, visited Coney Island, N. Y., and searched for infusorial earth and this time was fortunate enough to find it at Sheephead Bay, which is a village just on the Long Island side of Coney Island Creek. It was a grayish colored clay, one foot underneath the sand taken at low water, about eight feet from the surface of the soil. At Canarsie Landing, which is on Jamaica Bay between Coney Island and Auvergne, I did not find the infusorial earth, but I was there a very short time. I did find glacial phenomena and indication of the elevation of the coast, but of those I shall not speak now as they are not microscopical. But the finding of Bacillariaceæ in the infusorial earth, as belonging to the Upper Neocene period, is thus a fact, and the date of so finding is worthy of record. Perhaps they will be found more inland on Long Island hereafter. I have searched for them as far inland as the city of Jamaica, but without result.

This layer is in the Upper Neocene, or perhaps the Plistocene, but the placing of it definitely is extremely difficult if not impossible at present, for on describing a fossil marine Diatomaceous deposit from St. Augustine, Florida, Mr. Charles S. Boyer says (Bulletin of the Torry Botanical Club, April, 1895, Vol. 22, No. 4, page 172) that it, the St. Augustine deposit, "overlies an Eocene deposit and is beneath the Plistocene" and that the Barbadoes deposit, which corresponds partially with it, "is now claimed to be Pliocene." In fact, as I have already pointed out, the marine fossil layers of Bacillariaceæ, be it from Mors, Denmark; Simbirsk, Russia; Sentz Peter, Austria; Oran, Algiers; Moron, Spain; Argentina; Payta, Peru; New York to Virginia, California and New Zealand, including the Nicobar Islands, are Neocene, be that Miocene or Pliocene.

—Arthur M. Edwards, M. D., Newark, N. J.

The succession of Glacial changes.—Evidence has been accumulating during the last few years in favor of the periodicity of glacial action. Mr. Geikie recognized in Europe six distinct glacial epochs separated by genial periods, making in all eleven glacial and interglacial stages. For convenience he gives each of these horizons a separate name. The climax of glaciation was reached in the third

stage, that is, the second glacial epoch, after which the cold stage diminished continuously in importance. In like manner, the earliest interglacial epoch seems to have been the most genial, each successive epoch approximating more and more closely to existing conditions.

The American glacial deposits have been classified by Mr. Chamberlin, and an attempt made to correlate them with those of Europe. The following table shows the tentative correlation.

GLACIAL AND INTERGLACIAL STAGES.

European.		American.
XI.	Upper Tubarian=Sixth Glacial Period.	
X.	Upper Forestian=Fifth Interglacial Period.	
IX.	Lower Turbarian=Fifth Glacial Epoch.	
VIII.	Lower Forestian=Fourth Interglacial Epoch.	
VII.	Mecklenburgian=Fourth Glacial Epoch.	Wisconsin.
VI.	Neudeckian=Third Interglacial Epoch.	Toronto.
V.	Polandian=Third Glacial Epoch.	Iowan.
IV.	Helvetian=Second Interglacial Epoch	Aftonian.
III.	Saxonian=Second Glacial Epoch.	Kansas Formation.
II.	Norfolkian=First Interglacial Epoch.	
I.	Scanian=First Glacial Epoch.	

The complex series subsequent to the Wisconsin formation have not been sufficiently investigated to permit even a tentative correlation, or indeed, to even designate the specific formations. This statement is equally applicable to the formations deposited during the advancing stages of the glacial period in America. (Journ. Geol., Vol. III, 1895.)

Geologic News.—Paleozoic.—Haworth proposes to divide the Coal Measures of Kansas into Upper and Lower, the division to be at the top of the Pleasonton shales, which is at the bottom of the Erie limestone. The division is based principally on paleontological evidence. In the author's study of the Kansas Coal Measures he finds that the shales are of submarine origin, while the entire formation appears to have been laid down during a period of gentle oscillations, with the greatest movement to the west, and the least to the east. (Kan. Univ. Quar., Vol. III, 1895.)

An Orthoceras shell of gigantic proportions has been found in the Lower Coal Measures of Iowa, about forty miles from Des Moines. This specimen is three inches in diameter and as it is of the same very slender as the associated forms, it could not have been less than six feet in length, and probably was even longer. The species is *O. fauslerensis.* (Science, Jan., 1896.)

Mesozoic.—In examining the microscopic structure of the flint nodules found in the Lower Cretaceous of Texas near Austin, Mr. J. A. Merrill found traces of the following organisms: Foraminifera, sponges, molluscs represented by the nacreous tissue of the shells, and fishes represented by their scales. The fact that the delicate spines of the sponge spicules, even to the most minute barb are perfectly preserved, showing no trace of having been subjected to mechanical movement, leads to the conclusion, that these flints result from the continuous growth of sponges *in situ*. Mr. Merrill's study then confirms to this extent the view taken by Prof. Sollas in his study of the nodules of the English flint. (Bull. Harvard, Mus. Comp. Zool., Vol. XXVIII, 1895.)

Cenozoic.—Mr. G. H. Ashley's studies of the Coast Range Mts. of California lead him to the conclusion that the east and west ranges of Santa Barbara, Ventura and Los Angeles counties were elevated at about the end of the Miocene, while the ranges to the north with a uniform strike of northwest and southeast were elevated at or near the end of the Pliocene. (Geol. Mag., Vol. III, 1895.)

Mr. A. M. Edwards reports Cenozoic clay containing marine forms of diatomaceæ from Rockaway, Long Island. The clay deposit is dark green or grey in color, and is capped by a fresh water deposit of white clay. (Observer, Dec., 1895.)

Prof. H. L. Fairchild enumerates eight reasons for regarding the Pinnacles Hills, near Rochester, N. Y. as a kame series forming a part of a frontal moraine. This is contrary to the views of Upham who considers that they were deposited "in the ice-walled channel of a stream of water," "open to the sky." (Amer. Geol., Vol. XVI, 1895.)

BOTANY.[1]

A recent paper on the relation between the Ascomycetes and Basidiomycetes.[2]—In the October number of the *Revue Mycologique* under the heading "A Fungus simultaneously an Ascomycete and Basidiomycete" appears a résumé by R. Ferry of a portion of

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

[2] Read before the Botanical Seminar of the University of Nebraska, Dec. 21, 1895.

a paper published in *Mémoires couronnés de l'Académie de Belgique*, 1894 by Ch. Bommer. I have not seen the original paper, but as Ferry gives quite a lengthy account of it and quotes the most essential parts there seems to be sufficient basis for some remarks.

The fungi under consideration are *Mylitta australis* Berk. and *Polyporus mylittæ* Cooke and Massee. The former is a large irregularly spherical hypogeous fungous growth found in Australia and Van Diemans' Land and called by the inhabitants "native bread." It was first described by Berkeley in *Ann. and Mag. of Nat. Hist.*, 1839 and referred to *Mylitta*, a doubtful genus established by Fries upon what is now known to be a gall. Berkeley says he found no spores but noticed that the ends of some of the hyphæ were swollen. No one seems to have examined the fungus for some time after Berkeley described it. According to Ferry, Tulasne regarded it as a mycelial formation analagous to *Pietra fungifera* of Battara and older writers, which is now known to be the sclerotium stage of *Polyporus tuberaster* Fr. Later Cooke and Massee[3] referring to the plant incidentally call it a sclerotium and Saccardo[4] who examined it recently, says he observed spores (?) which were globose, smooth, hyaline, plainly nucleate and 14–15μ. in diameter. Such in brief was the knowledge of the plant before the appearance of the paper under discussion.

The latter plant *Polyporus mylittæ* C. & M. (fig. 1) was first described in *Grevillea* l.c. It is a short stipitate plant with a tough pulvinate pileus about 10 cm. broad, found growing on *Mylitta australis* in southern Australia. The authors say in a note; "A most interesting production, undoubtedly the ultimate development of the sclerotium long known as *Mylitta australis* Berk."

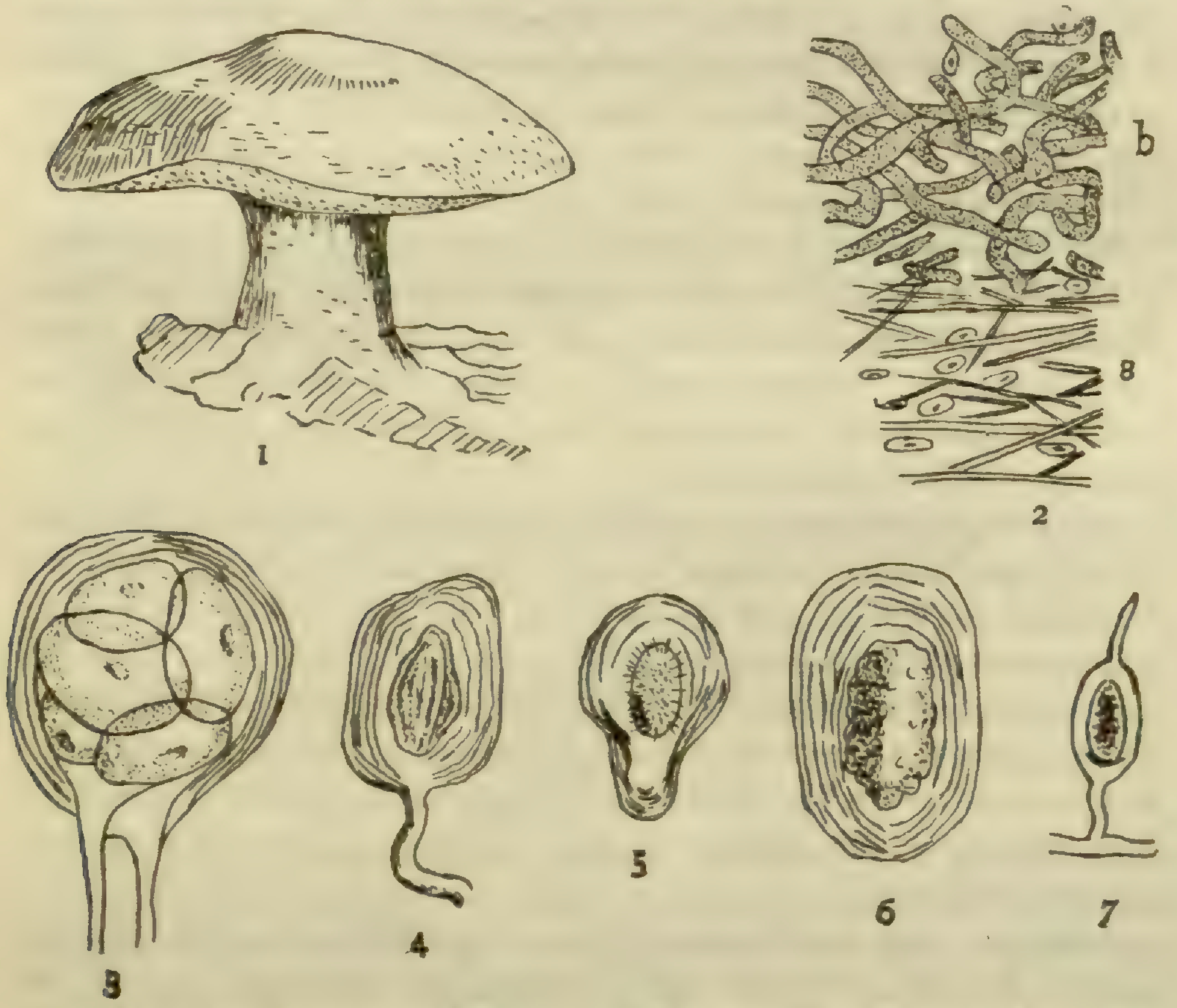
A year later Saccardo (l. c.) published a slightly different form of the same fungus under the same name. After the description he adds: "Growing on *Mylitta australis* from which it appears to originate. The texture of the *Polyporus* and of *Mylitta* are about the same. They are formed of intertwining filaments with frequent globose swellings constituting a soft or suberose white mass. It is very probable, therefore that *Mylitta* is the sclerotium form of the *Polyporus* and probably bears the same relation to the *Polyporus* that *Ceriomyces* bears to *Polyporus biennis* (Bull.) Fr."

Referring now to Bommer's paper we shall give the essential parts of Ferry's summary and translate the important parts of the quotations from the author. Ferry first gives an account of *Mylitta australis* as observed by Bommer.

[3] Cooke and Massee. Grev., **21**: 37. Dec., 1892.

[4] Saccardo. Hedw., **32**: 56. March and April, 1893.

Specimens are compact, very hard and covered with a superficial black crust. In full grown plants the interior is divided into a number of irregular cavities. The walls of these cavities are formed of a white tissue which under the microscope is seen to consist of thick-walled hyphæ which are stained by Bismark brown (fig. 2 b.). These hyphæ are from 4–8μ. in diameter. The cavities soon become filled with a gelatinous substance of a horny consistency in which some thin, hyaline, flexuose hyphæ are found buried. These are not colored by Bismark brown. Some of these hyphæ have ovoid swellings 5–8μ. long near their ends which contain 1, 2 or 3 ovoid bodies with very thin walls. Each body contains a kind of nucleus. Later these swellings (fig. 2 a.), especially those near the periphery of the gelatinous mass increase in size and contain only one ovoid body. This is brown, verrucose, very refringent, presenting all the characters of a spore and is regarded as such by Bommer. Since he finds what he considers asci and spores he refers *Mylitta australis* to the Tuberaceæ. He describes the



mature asci and spores as follows: "The asci (fig. 3) are analagous to those of *Tuber melanosporum*, being ovoid or spherical and 40–50μ. in their greatest diameter. The membrane is thin and encloses a single

elongated hyaline spore 20–30μ. long which is either smooth (fig. 4), verrucose (fig. 6) or echinulate (fig. 5)."

Nearer the centre of the gelatinous mass he says the asci are less plainly differentiated and frequently contain no spores (fig. 7). He submitted the fungus to chemical tests and found a great abundance of cellulose, but no glycogen, a substance usually present in Tubers.

The ordinary structure of the plant is, according to the author, as described above. As to its relation to *Polyporus mylittæ* which is frequently found growing from it, he says: "A specimen from the British Museum removes all doubt. This specimen like many others has a central cavity on one of the walls of which is seen a pulvinate mass formed by the hymenium of *Polyporus mylittæ*. This pulvinus does not possess true pores, but only small hemispherical cavities on its surface and numerous small rounded closed cavities in its interior which are covered by the hymenium. The mass ef hyphæ which forms the base of this hymenium is identical with the opaque white tissue which composes the walls of the cavities of nearly mature examples of *Mylitta*. Notwithstanding the presence of the pores and the thicker and more crowded hyphæ disposed after the manner of palisade tissue so characteristic of the hymenium of ordinary Hymenomycetes, the specimen is unfortunately sterile.

The particular disposition of the hymenium and the continuity and identity which exists between it and the sterile tissue of *Mylitta* establishes the fact that there exists between *Mylitta* and the *Polyporus* an intimate relation of the same nature as that which exists between the different stages in the life history of many fungi. Hence it follows that a carpophore of a Hymenomycete (*Polyporus mylittæ*) is here in reality the conidiophore of an Ascomycete (*Mylitta australis*). If this conclusion is true in the present case, it ought to be admitted that this is the relation which exists in general between the Basidiomycetes and Ascomycetes." ! ! !

Such are the author's preposterous conclusions, and thus is the autonomy of the Basidomycetes calmly disposed of.

One's first impulse is that this is a huge joke, but when you reflect that it emanates from the Belgian Academy of Sciences and is tacitly accepted by Ferry one of the editors of the *Revue Mycologique* the matter takes a serious aspect and it seems necessary to file a protest.

Ferry adds that DeBary long ago expressed the opinion that the Basidiomycetes and Uredineæ may be conidial forms of Ascomycetes. The only statement I find in DeBary[5] touching the point comes far

[5] DeBary, Comp. Morph. and Biol. of Fungi (Eng. trans.), p. 341.

from endorsing such an idea. The reviewer says further that Brefeld admits, theoretically, the existence of such fungi, but does not admit their actual existence because he does not think that two reproductive bodies of such complex development are able to be produced simultaneously. This idea Ferry regards as explaining the imperfect development of the *Polyporus* in Bommer's plant, and he adds, with a profundity equal to that of the author himself, that there are no existing characters which permit the plain separation of conidial bearing basidia (conidiophores) from typical basidia.

As to the author's observations, if accurately made they are of course deserving of consideration. We have not the space nor is it our purpose to discuss them here. Were it not for the fact that the normal form of *Polyporus mylittæ* is frequently found growing on *Mylitta*, and is regarded by several observers as being genetically connected with it, we might possibly disregard the supposed sterile hymenium and accept *Mylitta* as a tuber, though several of the author's observations are at variance with the characters of any known tuber. I am inclined for the present, however, to accept Tulasne's opinion and regard *Mylitta* as a sclerotium or a conidial stage of the *Polyporus*. As to the asci they may be illusions or may belong to some parasitic fungus. This is mere conjecture, however. These fungi are interesting forms and it is hoped that their study may be continued until the author's observations are confirmed or rejected. Accurate observations are always welcomed by botanists, but gratuitous and unfounded conclusions and generalizations should find no place in botanical literature.—C. L. Shear.

Polyporaceæ, Hydnaceæ, Helvellaceæ.—The undersigned desires species of the above groups from all parts of North America for the purpose of accumulating materials from which to monograph these families. In sending specimens, good representatives are desired, not mere fragments or abortive specimens. Where possible, indicate the host on which the fungus grows if a lignatile species, and especially in the case of fleshy or semi-fleshy forms, it is desirable to note the characters in a fresh condition. Even the most common species are desired in order to determine geographic distribution. When it is remembered that not a single species of any of these groups has been reported from more than half of our states and territories, it will be seen how great the necessity of coöperation on the part of local botanists and botanical collectors in order that this preliminary monograph may be as fairly representative of our flora as possible.

Before sending large packages, a preliminary correspondence will be desirable in order that the package can be sent the cheapest way. So

far as possible, specimens will be named for contributors and in all cases full credit will be given.—LUCIEN M. UNDERWOOD, Auburn, Ala.

The Smut of Indian-Corn (*Ustilago zeæ-mays*).—It has been found out at the Indiana Experiment Station that the smut does not attack the plant through the seed as has been supposed but like wheat rust it starts in the leaves and stems, wherever the spores are carried by the wind and find lodgment and sufficient moisture to enable them to germinate. The spores will grow as soon as ripe, that is as soon as the mass containing them turns black, and they will also retain their vitality for a year or two in case conditions for growth are not favorable.

It is evident from this that neither the time of planting nor the previous condition or treatment of the seed will have any effect upon the amount of smut in the crop. It is equally evident that meteorological conditions will have decided influence. Two things can be done to decrease smut in corn. The growing crop can be sprayed with a suitable fungicide and the entrance of the smut into the plant prevented. That this can be made effective is shown by experiments at the Indiana station. The other, more convenient but less thorough, method, is to gather and destroy the smut, and thus eventually rid the fields of it.—(*Bull. Ind. Station.*)

Antidromy and Crossfertilization.—I have been much interested in Dr. Macloskie's article on Antidromy in the November number of THE NATURALIST. It reminds me of some observations which I made several years since while investigating the subject of crossfertilization. They will be found recorded in the same journal August, 1880. A suggestion is ventured as to the possible cause of it in the flowers of *Saxifraga sarmentosa* on pages 573 and 574 of that number. In that case it seems to have little or no value in aiding crossfertilization.

Other cases, however, have been noted where it seems probable that it may be of essential value in that direction, viz., in *Solanum rostratum* and *Cassia chamaecrista.* They show lateral asymmetry, by which the pistil is on opposite sides, in successive flowers of a cluster. These plants will be found described also in THE AMERICAN NATURALIST, April, 1882.

It may be an item of general interest, also, that the features of the flowers there pointed out are so remarkable as to have attracted the attention of Darwin. He addressed a letter of congratulation and inquiry to the writer with his characteristic candor and cordiality. It may have been the last letter from that illustrious hand, for he lay cold

in death before the missive had reached its destination. He called attention also to the fact that he had observed similar asymmetry in *Mormodes ignea* and had similarly used the terms " right handed " and " left handed." The fact is published in his " Fertilization of Orchids." —J. E. TODD.

University of South Dakota, Vermillion, S. D., Dec. 2, 1895.

VEGETABLE PHYSIOLOGY.

Water Pores.—Dr. Anton Nestler contributes an interesting " Kritische Untersuchungen über die sogenannten Wasserspalten " (pp. 38, pl. 2) to Band LXIV, No. 3 of *Nova Acta d. Ksl. Leop.-Carol. Deutschen Akad. d. Naturforscher.* The term " water pore " was introduced by DeBary to designate a mechanism supposed to be distinguished from ordinary stomata by (1) Presence of liquid water, at least at times, in the substomatic opening; (2) Rigid guard cells; (3) Often very considerable differences in form and size; (4) Location near the edge of the leaf in the teeth over the end of a vascular bundle. The following subjects are considered in this paper: Previous literature; development of the water pores; structure, number, and size; rigidity of the guard cells; plants destitute of water pores. Dr. Nestler shows that water pores originate from stomatic mother cells in the same way as ordinary stomata (48 species of Ranunculus were examined and also plants of many other families); that while water pores sometimes exceed ordinary stomata in size they are quite as often of the same size or smaller, and frequently show plain transitions into the latter; that rigidity of the guard cells is not always present in the water pores nor always absent in ordinary stomata; that water pores sometimes discharge vapor of water; and, finally, that the ordinary stomata sometimes, and probably often, excrete liquid water (over the whole upper surface of the leaf in *Vicia Faba*).—ERWIN F. SMITH.

Biology of Smut Fungi.—The third part of Dr. Brefeld's Smut Fungi (Heft XII of the *Untersuchungen*) contains 140 pages of quarto text and 267 figures packed into 7 lithographic plates, the crowding together of which makes difficult the comparison of text and figures. All told 13 genera and 64 species are described, of which latter 22 are reckoned as new. The germination of the smut spores is figured for most of the species as well as described. The descriptions are long and include a wealth of biological detail drawn from the behavior of the

various forms in Nährlösung. Two new genera are established: Arthrocoidea, founded on two old species (*Ustilago subinclusa* and *U. carycis*), separated from Ustilago by peculiarities of germination, and Ustilaginoidea, a most peculiar genus, founded on Patouillard's *Tilletia oryzœ* and on a new species found by Möller on *Setaria Crus-Ardeœ* in Brazil. Material for the study of the fungus on rice was obtained from Barclay in India. This fungus which causes a swelling of the ovaries of the rice plant to several times the normal breadth of the grain and which has the external appearance of a smut, has nothing to do with Tilletia, but seems to belong to some other group of fungi. Its principal peculiarities are (1) the production of a large number of smut-like spores on the outer part of the transformed grain, the interior of the same being occupied by a hard mass of nonsporiferous hyphæ suggesting an immature sclerotium; (2) germination in a manner totally different from that of any other smut spores and resembling that of some Ascomycetes, i. e. by the development of a much branched septate mycelium which, in dilute Nährlösung, bears succedaneously on the ends of the hyphæ, small, oval, colorless, nongerminating conidia, and in concentrated Nährlösung omits these conidia and develops in their stead and also anywhere on the walls of the hyphæ, sessile dark greenish-black, echinulate, thickwalled spores one in a place or sometimes two together, one above the other. In the species received from Brazil most of the dark spores had fallen off and the development of the central mass of hyphæ had proceeded a step further, being changed into a true sclerotium with a black rind and an internal thickwalled white pseudoparenchyma. Additional facts are promised as soon as these sclerotia can be induced to germinate. The descriptions are followed by a discussion of the relationship of the smuts to each other and to other fungi. A full account of culture methods and some additional notes on fungi are promised for Heft XIII to appear soon.

Incidentally Dr. Brefeld pays his compliments to the perfunctory grinders out of species: "The accidental circumstance that the all naming Patouillard has given to the fungus on rice the name *Tilletia oryzœ* shows once more how worthless are the namings of a spore material without the developmental history. The latter shows that in Patouillard's supposed *Tilletia oryzœ* we have to do not with a Tilletia and not even with a smut fungus but with a form out of the highest group of fungi." This is quite to the point. The labors of the "all naming" mycologists of the past have filled this part of systematic botany with a mass of rubbish mountain high, and still the brave work goes on, exactly as if it were not known that fungi are exceedingly

variable organisms, or that it is possible by holding on to the old notion of fixity of species to make half a dozen new ones out of the product of a single spore by a little variation of the substratum, or even without the latter device by drawing up separate descriptions of old and young and large, small and medium sized spores. Is it not indeed time we should have a reform and begin to *reduce* the number of species by carefully studying those which have been badly described (by far the larger number), learning their life history and the extent of their variability under ordinary conditions, and throwing out the synonyms? This method carefully applied would unquestionably reduce the number of so-called species of fungi and bacteria nearly or quite one-half. This must necessarily form a large part of the work of the next generation of mycologists, and no one familiar with the ground can doubt that the task of properly classifying these plants would be immensely easier if half the descriptions had never been written.—Erwin F. Smith.

Function of Anthocyan.—The following is an abstract of a short paper by Prof. Leopold Kny, of Berlin, Zur physiologische Bedeutung des Anthocyans, published in *Atti del Congresso Botanico internazionale di Genova*, 1892 (pp. 135–144). The name anthocyan has been given to a coloring matter occurring in the vegetative and floral organs of many plants in numerous transitional shades from red through violet to blue. It occurs dissolved in the cell sap and is sensitive to acids and alkalies, changing from one shade of color to another as they are used. It is probable that several different substances have been included under this term, for while in most plants these colors appear only on exposure to light, especially bright sunshine, in others they appear just the same in total darkness, e. g. in the perianth of *Tulipa gesneriana, Crocus vernus,* and *Scilla siberica,* the inner tissues of the root of the red beet, and the inner leaves of the red cabbage. In case of the floral organs anthocyan undoubtedly serves to make them conspicuous to insects, etc., but for the most part it can have no such function in the vegetative organs. Its use to these parts of the plant has been explained in three different ways. (1) When young leaves and stems either from seedlings or from buds take on a distinct red or violet color and subsequently lose it wholly or in part, it is but a step to the hypothesis that this color has been developed for the protection of the chlorophyll from injury by light. It is explained in this way by Kerner von Marilaun. On this supposition, it is difficult to understand how many young shoots get along without it, e. g. species of Iris, the young leaves of which are bright green. As proof, Kerner makes

prominent the abundance of anthocyan in many alpine plants as well as the fact that when a species grows on the plains as well as in the mountains it is in the latter locality that the vegetative and floral organs show an inclination to become red with anthocyan. (2) In cases where the cells holding the anthocyan are on the under side of the leaf, the upper side being pure green (*Cyclamen europœum, Hydrocharis Morsus ranœ*) the lightscreen hypothesis naturally falls to the ground. Here there is every reason to believe, according to Kerner, that the light rays which would otherwise pass out of the plant and be lost are converted into heat rays in passing through the cells containing anthocyan. In conformity with this hypothesis we find that the leaves of trees and shrubs which are lifted up from the soil and have other green leaves below to catch the filtered light, are never violet on their under surface, while, in very leafy under shrubs, only the lowermost leaves next the ground are provided with anthocyan. Another indication of the warming influence of anthocyan is its abundance in alpine plants, as already mentioned, and its frequent development in the perennial leaves of other plants during the winter season (*Sempervivum tectorum, Ligustrum vulgare, Hedera helix, Mahonia aquifolium*) the leaves being enabled thereby, in sunny winter days, to break up carbon dioxide even at relatively low temperatures. (3) There are, however, a series of facts going to show that the preceding hypotheses are not sufficient to explain all cases. On full grown shoots of many herbs and woody plants the sunny side of the internodes frequently becomes red while the opposite side remains nearly or quite pure green (*Salix* species, *Polygonum fagopyrum*, and many other plants). The same difference is frequently observed on petioles, the red color being not rarely prolonged into the midrib and its branches. These facts lead to the conclusion that the screen of anthocyan may have some use in connection with the breaking up and translocation of plastic substances through the vascular system. This is also indicated by the fact that when the roots of willows and other plants grow down from a bank into the water and are subject to direct sunlight they become red on the exposed surface. Pick considers the anthocyan screen as a means of bringing about the outward movement of starch in large quantities without seriously disturbing the assimilatory activity of the chlorophyll bodies. Some effort has been made to demonstrate this third view, but so far as known, no one has tried to establish the first two by means of experiment. The following experiments were, therefore, undertaken to fill this gap. (1) *Does anthocyan protect chlorophyll from the destructive action of light?* Owing to the manifest difficulty of dealing directly with the chlorophyll bodies the experiments were made

with an alcoholic solution derived from grass leaves. Two beakers were filled with this green solution and placed in tin chambers with blackened inner walls but having on one side a quadrangular opening with strongly projecting edges for the entrance of light. In front of each opening was placed a parallel walled glass vessel 196 millimeters high, 93.5 mm. wide and 40 mm. thick. Into one of these vessels red beet juice was poured and into the other white beet juice, both filtered and of the same specific gravity. The result was decisive. The light which passed through the anthocyan solution discolored the chlorophyll much less rapidly than that which passed through the colorless solution. (2) *Does anthocyan convert the light rays into heat rays?* Experiments were made with the foliage of green and red leaved varieties of the following species, viz. *Fagus sylvatica, Corylus avellana, Berberis vulgaris, Acer platanoides, Brassica oleracea, Dracæna ferrea, Canna indica;* with decoctions of white and red beets; and with the petals of a white and a red rose. Exactly weighed quantities of the leaves, etc., were placed in the parallel walled glass vessels already mentioned, thermometers were then plunged into the center of the mass, and the vessels were exposed to the action of direct sunlight filtered through a nearly saturated alum water screen 4 cm. thick (to absorb the heat rays). In most of the species (*Dracæna ferrea* and *Canna indica* gave contradictory results) the ability of anthocyan to convert light rays into heat rays seems to have been demonstrated conclusively. In one to two minutes in favorable cases there was a rise of temperature in the vessels containing the red leaves, the maximum difference amounting to as much as 4°C. As soon as the sun was covered by a cloud there was a noticeable fall of temperature in both vessels, and when the cloudiness lasted 10 to 20 minutes the temperature became the same or nearly the same in both vessels. Subsequently an effort was made to determine whether the different light rays of the solar spectrum behaved differently. For this purpose three vessels containing, in turn, red leaves of several species of plants were exposed to direct light under the following conditions; the light entering one vessel was filtered through the alum solution, that entering another was filtered through a screen of sulfuric-copper-oxide-ammonia, that entering the third was passed through a solution of bichromate of potash, it having been determined in advance spectroscopically that the two colored screens divided the spectrum in about the middle of the green. Under these conditions the rise of temperature was less behind the blue screen than behind the orange one, and less behind the latter than behind the alum screen. A consideration of the third supposed function of anthocyan is left by Dr. Kny for a subsequent paper.—Erwin F. Smith.

ZOOLOGY.

A posthumous paper on Myxospridia by M. Prosper Thélohan has recently appeared prefaced with a short account of the authors's scientific career by E. G. Balbiani. The Memoir, intended as a thesis for the degree of Doctor of Science, while complete in the essential parts, lacks the final chapter in which the author intended to indicate the relations of the different genera and families of the Myxosporidies.

Briefly stated, Myxosporida are parasitic Sporozoa found living in certain fishes, batrachians and reptiles. They have also been observed living in various arthropods, notably spiders and crustaceans. Certain families are limited to vertebrates host : Myxobolidæ and Chloromyxidæ. It is to the latter forms that the author devotes his paper.

It has long been known that the Myxosporida of the vertebrates assume two forms; one, a small ameboid body swimming free in the liquid which contained in certain organs, chiefly the gall and urinary bladders, and a second form which is found distributed in compact tissues, like the connective tissues and the muscles. In either case they may be harmless to the host, or on the other hand, give rise to grave disorders, resulting in the death of the animal which they have invaded.

The free swimming species are variable in form, the most common one being that of an elongated cone the base of which corresponds to the anterior extremity; others are almost spherical. It is, however, difficult to decide upon a definite species form, since each individual exhibits such extraordinary polymorphism. The organisms found in the tissues are generally spherical.

Ordinarily these parasites are colorless, but yellow ones have been seen, and a few green ones are reported.

In dimensions, as in form, there is great diversity. The free swimming species are from 10 or 12μ. in diameter to 5 mm. in diameter.

Reproduction is accomplished by sporulation, and, probably, also by fission. The protoplasmic body of the Myxosporida is plainly differentiated into a peripheral zone, *ectoplasm*, surrounding the central sarcode, *endoplasm*. The former functions as a protection for the latter and, also is capable of putting out pseudopodia which act as organs of locomotion or fixation. These pseudopodia are localized in certain species, in others they appear at random. They take no part in the phenomena of nutrition.

The endoplasm of young individuals appears homogeneous, but in older ones there are found, in some cases, certain products of differentia-

tion, among which the author distinguished, fatty globular masses and rhombohedral crystals of hæmatoidin. In others, there are vacuoles, containing protoplasmic matter which differs from the rest of the endoplasm. It is in the endoplasm also that the nuclear elements are found, often in great number, around which the spores develop. The author traces the development of these spores, describing minutely the various stages of growth. Upon arriving at maturity they remain enclosed in the endoplasm for a varying length of time. When set free it seems to be connected with the destruction of the protoplasm which persists in the mother organism after the formation of the spores. The free-swimming species are expelled from the host either with the fæces or the urine, but the ones imprisoned in the tissues continue where they are until set free by the death and subsequent decay of the tissues of the host. The spores rarely germinate in the old host, never in any exterior medium, but stay dormant until chance provides them a new host.

As to the food habits of the Myxosporida, M. Thélohan observations are to the effect that they imbibe nourishment from the fluids in which they live. In no case did he see food particles ingested.

The following classification of the Myxosporida was proposed by the author in 1892, and his subsequent researches confirms the distinguishing characters.

Spores	form variable	no vacuoles in the plasma.	2 capsules.	I. Myxidiidæ.
			4 capsules.	II. Chloromyxidæ.
		1 vacuole which colors a reddish brown by iodine.		III. Myxobolidæ.
	pyriform, a single polar capsule, not easily seen, with a pointed extremity; a clear vacuole, not colorable with iodine, at the larger end.			IV. Glugeidæ.

Myxidiidæ contains 6 genera with 25 species; Chloromyxidæ has 1 genus, 6 species; Myxobolidæ 2 genera, 14 species; Glugeidæ 3 genera, 16 species.

The Segmentation of the Hexapod Body.—In a recent paper[1] giving the results of work upon the early stages of certain of the Orthoptera, Dr. Heymons gives the whole number of segments in the Hexapod body as twenty-one, of which six form the head; three, the

[1] Anhang. Abh. K. preuss. Akad. Wiss., Berlin, 1895.

thorax; and twelve, the abdomen. At some time during the development of the insect, appendages are present upon all except the first, third and twenty-first segments. The frons, clypeus, labrum and compound eyes are parts of the first segment. The second segment bears the antennæ, the fourth the mandibles, and the fifth and sixth the two pairs of maxillæ. The hypopharynx does not belong in the series of appendages but is formed by a folding of the ventral portions of the fourth, fifth and sixth segments. The cerci, contrary to the views of some authors, are the true appendages of the twentieth (eleventh abdominal) segment. Considerable emphasis is laid upon the similarity between the first and twenty-first segments, in their relations to the openings of the alimentary canal, in being free from appendages, in the lateral position of their ganglia and in the relative changes of the appendages of the adjoining segment. Concerning the position of the genital openings, Heymons reiterates his former opinion that they may belong primitively to the tenth segment, their position in the ninth being a secondary development.—G. M. Winslow.

The Coxal Glands of Thelyphonus caudatus.—In a brief note in the Zoologischer Anzeiger,[2] Dr. Theo. Adensamer adds a few facts to complete Sturany's work on the Arachnoidea. The two glands occur between the gastric cœca and the muscles, and extend as unbranched and unlobed sacs to the abdomen. From the anterior end of each extends a simple duct to the coxæ of the first pair of legs through which they open. A thin chitinous intima was distinguished in the ducts. An histologically differentiated portion of the gland corresponding to Lankester's medullary substance and Sturany's Marksubsanz was not found.

The following table shows the location of the openings of the glands in the several groups:

Limulus, openings in the 5th appendages.
Scorpio, openings in the 3d pair of legs = 6th appendages.
Pseudoscorpionidea, openings in the ? = ?
Thelyphonus, openings in the 1st pair of legs = 3d appendages.
Araneida:
  a. Tetrapneumous, openings in the 3d pair of legs = 5th appendages.
  b. Dipneumous, openings in the 1st pair of legs = 3d appendages.
Phalangida, openings in the 3d pair of legs = 5th appendages.
Acarina, openings in the ? pair of legs = ? —F. C. K.

[2] XVIII, p. 424.

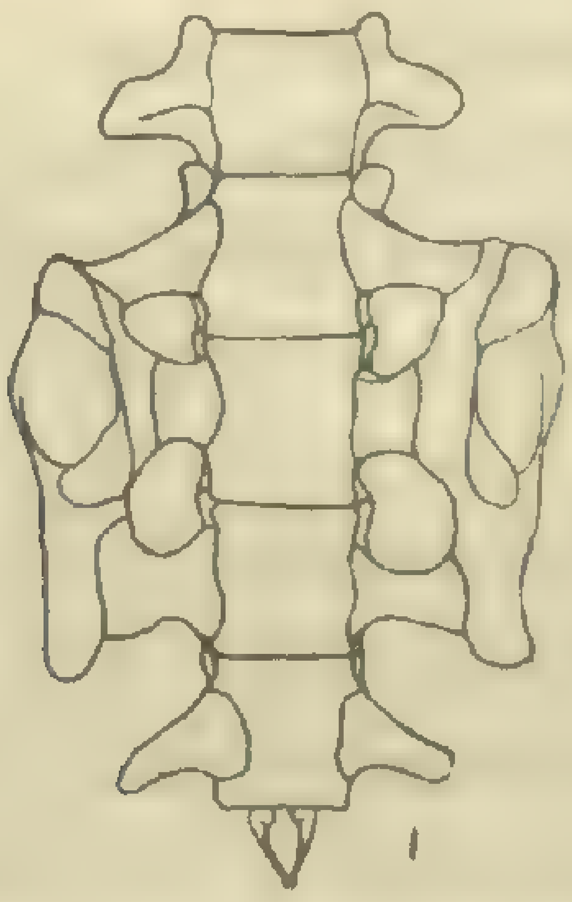
Cross Fertilization and Sexual Rights and Lefts Among Vertebrates.—The November number of this journal, page 1012, under the title "Sexual Rights and Lefts," called attention to sexual peculiarities I had recently discovered in certain Cyprinodonts. At that time no satisfactory explanation of the purpose or origin of the strange conditions offered itself. At present I would like to note in these pages what upon further consideration appears to me the best solution of the problem. Additional study has satisfied me that the sexual conditions in the genus Anableps prevent close "inbreeding," or, in other words, they secure cross fertilization. What in certain plants is attained by means of short stamens with the long ones is in these fishes realized by sinistral and dextral males and females. This is a view in the case of Anableps that brings us in face of probable benefit from the novel features, and of the possible causes of their evolution. As bearing on the inception of the dextral and the sinistral peculiarities we must consider the habit possessed by so many of these fishes of swimming in pairs, side by side, a habit that induced Professor Agassiz to name one of the genera *Zygonectes*, that is yoke swimmers. The acquisition of more or less of a dextral or of a sinistral tendency would not be at all unnatural in each of a pair habitually swimming side by side in the same relative positions to one another. It may be that cross fertilization will afford an explanation of conditions somewhat similar among molluscs.

While writing of matters concerning the publication "The Cyprinodonts," it should be mentioned, as kindly pointed out to me by Dr. A. Smith Woodward of the British Museum, that the name of one of the new genera, *Glaridodon*, was recently preoccupied among fossils, and it may be well here to discard that name (p. 40) for the term *Glaridichthys*.—S. Garman, Cambridge, Mass.

Abnormal Sacrum in an Alligator.—Among a lot of young alligators procured from New Orleans for the University of Chicago one in which the skeleton was prepared, showed a very peculiar variation in the pelvic region there being three instead of two sacral vertebræ.

There are as usual 24 presacral vertebræ. The 25th has the sacral ribs inclined backwards and becoming slender. The 26th has strong thick ribs, and the 27th, the first caudal in normal specimens, has also well developed ribs articulating strongly with the ilium. The 27th is seemingly biconvex. The first chevron is attached between the 28th and 29th and is, therefore, in the normal position as regards the serial number of the vertebræ, but is attached to the first vertebræ the last sacral instead of the second. The whole pelvis has migrated backwards one

vertebræ, the first true sacral tending to become a lumbar and the first caudal has become a sacral. The two side are strikingly symmetrical. The figures giving views from above and below are natural size and include the 24th–28th vertebræ.



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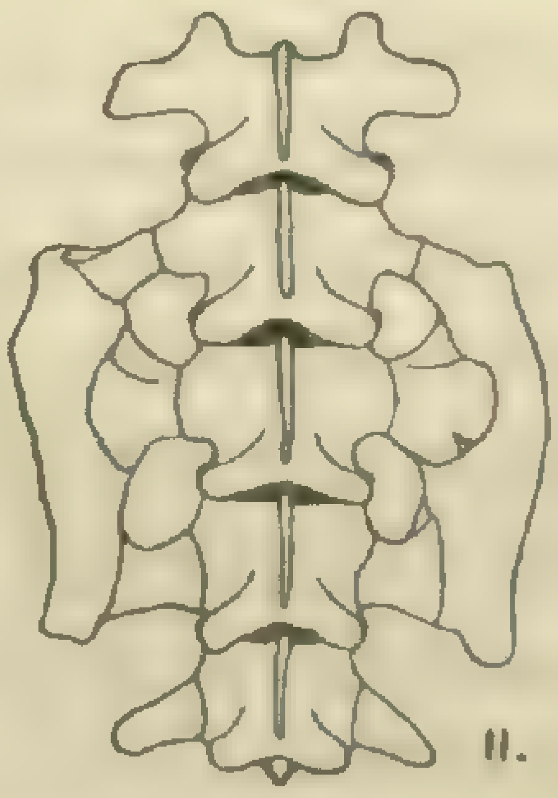

II.

The other known cases of variation in the sacrum of Crocodilia are, as far as I am aware, as follows: Rheinhardt[1] examined 11 specimens and found 3 abnormal.

1. *Alligator sclerops* Schn.: Last lumbar become a sacral; 23 presacrals.

2. *Crocodilus acutus:* 3 sacrals, 3 plane-convex, 1st caudal concave-convex and bearing a chevron, thus the first caudal has become a sacral, 23 presacrals.

3. *Crocodilus acutus:* First caudal has become a sacral, 24 presacral.

Baur[2] reported two cases.

1. *Gavialis gangeticus:* 25 presacrals. One intercalated between the 9th and 10th.

2. *Alligator mississippiensis*: Last lumbar become a sacral, showing on one side a small sacral rib and which does not reach ilium, 23 presacrals.

Baur[3] reported three cases.

1. *Crocodilus acutus:* A specimen in the museum at Cambridge, Eng. shows on the right side of the 25th vertebra a strong and separate rib, on the left side the rib is smaller and coössified with the centrum. The 26 shows typical sacral ribs. The 27th shows on the left side a

[1] (Anomalier i Krydsvirvlerne hos Krokodelerne, Copenhagen, 1873, and Sur les anomalies des vertèbres sacrées chez les crocodilieus. Jul. de Zoologie T. III, No. 4. Paris, 1874.)

[2] Zoologischer Anzeiger, IX Jahrg., No. 238, 1886. Osteolog. Not. über Reptilen.

[3] (Zoolog. Anz. XII, Jahrg., No. 306, 1889. Revision meiner Mittheilungen in Zoologisher Anzeiger, mit Nachträgen.)

strong free rib and on the right side a weaker rib but free. The 28th biconvex.

2. *Crocodilus acutus*: Two specimens in the Royal museum at Leiden have only 23 presacrals.—E. C. CASE.

The Polar Hares of Eastern North America, with Descriptions of New Forms.—In 1819 Captain John Ross, in the fourth Appendix of the second (octavo) edition of his "Voyage of Discovery" in Baffin's Bay, described a hare which he procured in Baffin Land, in latitude 73° 37′.

To this animal he gave the name "*Lepus arcticus* Leach," stating at the end of his description that "Dr. Leach thinks it to be very distinct from the common White Hare of Scotland (*Lepus albus* Brisson) and equally so from the *Lepus variabilis*, Pallas." Ross then makes a reference to "Appendix No. V," of the same volume, which he evidently supposed would contain Leach's description of the same animal. Leach's chapter on the "New Species of Animals" obtained by Ross, however, does not come in appendix number five but is part of the same appendix in which Ross' description appears. It is on page 170, while Ross' description is on page 151. Leach evidently described the same specimen which Ross had in hand, but gave it the name *Lepus glacialis*. Owing to its precedence in paging, Dr. J. A. Allen[1] rightly adopts the name *arcticus* for the American Polar Hare, *glacialis* of Leach becoming a synonym.

The question has been raised by my friend, Mr. Outram Bangs, whether Ross, and not Leach, should have credit for the name *arcticus*. We may justly infer from Ross' description that he intended that Leach should have this credit and that he published it with such intention. He must have consulted with Leach about its relations to the European and Scottish Hares and quotes Leach in his diagnosis, using, without doubt, the specific name then suggested by Leach. The fact that Leach gave it another name does not affect the status of the one given by Ross, nor weaken Leach's claim to it. From the present custom, not definitely formulated in our American Ornithologist's Union's canons of nomenclature, I see, however, no alternative but to call the Baffin Land Hare, *Lepus arcticus* Ross.[2]

[1] Mon. N. Amer. Rod., 1877, p. 288.

[2] Some authorities prefer that sole credit for the name of a species be given to the person to whom the original publisher of that name ascribes the origin of the name, writing it in this case *Lepus arcticus* Leach. The A. O. U., with one (or two ?) exceptions, adopts the reverse rule in their check list of birds, and would make it read *Lepus arcticus* Ross. Neither method does justice either to the public or to

Dr. J. A. Allen (l. c.) concludes that the American Polar Hare is not specifically separable from the European *L. timidus* (=*variabilis* Auct.), and the deficient material which he had for examination at that time probably justified such a verdict as the safest one, especially when we consider the standard of species and varieties adopted at that date by American mammalogists. Through the kind liberality of Messrs. G. Brown Goode and F. W. True of the Smithsonian Institution, and of Mr. Outram Bangs of Boston, I have been favored to examine, in connection with the specimens in the Academy of Natural Sciences of Philadelphia, an unusually large series of skins and skulls of the Polar Hares of America and northwestern Europe. The results of this study, so far as they relate to the Polar Hares of eastern North America, and Scandinavia may be summed thus briefly.—

1. Lepus timidus L. Scandinavian Polar Hare.

Type locality (hypothetically restricted), Southern Sweden.

Nasals nearly or quite reaching to anterior vertical plane of premaxillaries. Posterior frontal swelling on a plane with the postorbital processes. Upper incisor with transverse sectional diameter greater than the longitudinal diameter; the chord of the arc of its exposed surface (with skull, minus mandibles, resting on a plane horizontal surface) is vertical; the radius of the arc described by the incisors is one-eighth ($\frac{12}{100}$) of the basilar length of skull; their inner faces indented by a deep broad sulcus and they are rooted on the premaxillaries at or slightly anterior to the inferior maxillo-premaxillary sutures. Roots of lower incisors extending to base of pm. 1.

Summer pelage; above blackish brown, sprinkled with gray; ears darker, but not black, tail white, dark above.

2. Lepus arcticus "Leach," Ross. Baffin Land Polar Hare.

Type locality, lat. 73° 37′, northern Baffin Land, southeast of Cape Bowen.

Size larger (?) than *timidus*, with relatively smaller and wider skull and shorter ears. Skull of the same type as *timidus*, with the following differences: Nasals, rostrum and incisive foramina relatively

those personally interested. I suggested (Proc. Acad. Nat. Sci., Phila, 1895, p. 395), that both the publishing and the manuscript or verbal authority for such names should be indicated. My friend, Witmer Stone, has suggested an improvement on my formula which I heartily endorse, viz.. that instead of "*Rana clamitans* Bosc., Mss., Sonn., Latr." (l. c.), it should read *Rana clamitans* "Bosc.," Sonn. & Latr., and the Baffin Land Hare would read *Lepus arcticus* "Leach," Ross. This comports far better with our motto that, "Zoological nomenclature is a means, not an end, in zoological science."

shorter and broader, the incisive foramina never reaching to middle of pm. 1. Palatal bridge longer than width of postpalatal fossa. Supra orbital processes of frontals deeply notched anteriorly, upraised and widely flaring. Frontals, at their posterior constriction, remarkably tumid, their anterior plane greatly depressed.

Summer pelage (fide Ross and Leach (l. c.) and Sabine[3]), white, "The back and top of the head are sprinkled with blackish brown hair which is banded with white, the sides of the neck are covered with hairs of the same color, interspersed with white. The extreme tips of the ears are tipped with black."—Leach. "In some of the full-grown specimens, killed in the height of summer, the hair was a grayish brown towards the points but the mass of fur beneath still remained white, the face and front of the ears were a deeper gray."—Sabine.

In south Baffin Land, as evidenced by a specimen from Cumberland Gulf, the type form intergrades into the following subspecies:

3. Lepus arcticus bangsii Rhoads, subsp. nov. Newfoundland Polar Hare. Type locality, Codry, Newfoundland. (Diagnosis as given below.)

4. Lepus grœnlandicus Rhoads, sp. nov. Greenland Polar Hare. Type locality, Robinson's Bay, Greenland. (Diagnosis as given below.)

Lepus arcticus bangsii,[4] subsp. nov. Newfoundland Polar Hare.

Type, Ad. ♀, No. 3752; Col. of E. A. & O. Bangs. Collected by Ernest Doane at Codry, Newfoundland, Aug. 3d, 1895.

Description.—Size equal to *L. timidus* L., of Southern Sweden, with shorter ears, shorter and broader skull, nasal bones and incisive foramina, weaker dentition and narrower frontal breadth anterior to the supraorbital processes.

Adult summer pelage: entire back and upper sides, including neck, shoulders and outer surfaces of thighs, uniform dark grizzled gray, faintly suffused with tawny. A pinch of hairs from near middle of back shows the following color pattern: under-fur fine, tawny-white basally, becoming tawny at distal end; over-fur white or black at base in about equal proportions, the coarser black-based hairs black throughout, the finer white-based hairs with terminal half black, interrupted by a subterminal band of white or pale tawny. Lower head

[3] Suppl. Appx., Parry's Voy., 1824, pp. 187–188.

[4] Named for Mr. Outram Bangs, who has done so much in making known the mammal fauna of Newfoundland, and who has there collected the finest study-series of Polar hares that can be found in this country.

(including chin), lower neck, nape, forebreast to forelegs, lower sides, edges of thighs and rump, dark plumbeous gray, flecked with very long, slender, white hairs. Lower breast, belly, vent and tail white, bordered by a nearly clear plumbeous edging which separates the ventral from the abdominal regions and joins the dark rump along the inside of thighs. Inner anterior border of hams, sides of hind feet and toes, and lower surfaces of forelegs, white, thinly intermixed with leaden hairs. Outer surfaces of fore and hind legs and superior surfaces of the feet, tawny gray. Ears and space between them, black, becoming grayish at base and with a narrow whitish outer posterior margin from near base to tip. Upper head, including cheeks and nose, grizzled buffy gray, appreciably lighter than the gray shades of the back. Eyelids whitish, edged with black. Whiskers weak and sparse, white and black in equal proportions, the longer black hairs tipped with white.

Winter pelage (No. 1187, Ad. ♀, Col. of E. A. & O. Bangs. Bay St. George, Newfoundland, Mar. 1, 1895): Entire pelage, exclusive of ears, white. Extreme tips of ears black, the median anterior borders of ears grayish.

Measurements (of type).—Total length, 626 millimeters; tail vertebræ, 63; hind foot, 160; ear (from crown), 85. Skull: total length, 97; basilar length, 76; greatest breadth, 48.2; anterior frontal constriction, 23; length of nasal (longest diagonal), 40; greatest breadth of nasals, 22; alveolar breadth of upper incisors, 9; greatest length of mandible, 76; greatest width of mandible, 47.

The above measurements both of body and skull are a very fair average of the dimensions of five adults taken for Mr. Bangs in Newfoundland by the same collector, Mr. Ernest Doane. Summer specimens from northern Labrador are inseparable from those taken in the same month in Newfoundland. A summer series from Great Slave Lake may show the existence of another race of *arcticus* in that region.

LEPUS GRŒNLANDICUS sp. nov. Greenland Polar Hare.

Tpye, No. 1486 ad. ♂. Col. of Acad. Nat. Sciences, Philadelphia. Collected by Chas. E. Hite at Robinson's Bay, North Greenland, Aug. 2, 1892, for the Peary Relief Expedition.

Description.—Size larger than *L. timidus* L. of Sweden, with radically distinct coloration and incisor dentition.

Adult summer pelage, white, suffused with light tawny and sparingly sprinkled with gray on upper head and ears. Back with scattering black and gray hairs. Tip of ears black. Tail, sides and lower sur-

faces pure white. Half grown young in July and August like adult, but darker, owing to greater abundance of colored hairs and the leaden under fur. Appearances of young and old at a distance at all seasons, white.

Winter pelage, pure white throughout, except the tips of ears, which are black.

Skull with rostral portion anterior to pm. 1, relatively much longer and more attenuate, owing to the outward prolongation of the premaxillaries and the small calibre of incisors. Upper and lower incisors very long, produced and slender, their transverse diameter being less than the longitudinal. Upper incisors describe the arc of a circle whose radius is one-fifth ($\frac{20}{100}$) the basilar length of the skull. The chord of their exposed arcs (with cranium, minus mandibles, resting on a plane horizontal surface) forms an angle of 45° to the horizontal plane. Face of upper incisors multistriate, the normal sulcus peculiar to all other members of the genus being so filled with dentine in adult *grœnlandicus* as to obliterate the depression, presenting an even, rounded, enameled contour marked with three minute striæ.

Roots of upper incisors based on the maxillaries and reaching back nearly half way from inferior maxillo-premaxillary sutures to pm. 1. Roots of lower incisors extending to the base of pm. 2.

Measurements (of type taken from dry mounted skin, relaxed): ear, from crown, 100 millimeters; hind foot, 145; tail vertebræ (dry), 50?

Skull: total length, 100; basilar length, 84.5; greatest breadth, 50; anterior frontal constriction, 22.5; length of nasals (longest diagonal), 41; greatest breadth of nasals, 20.5; alveolar breadth of upper incisors, 8.5; greatest length of mandible, 76; greatest width of mandible, 48.

Five skins, seven skulls, and one skeleton, all from North Greenland, comprise the Academy series of Greenland Hares, and all confirm the peculiar characters of this species as above given. I regret that more complete body measurements are not available. Average adult measurements of ear and hind foot are 100 millimeters for the former and 145 for the latter. The total length of an adult skeleton (ligamentous) is 519 millimeters, measured as in the flesh, from tip of nose to end of tail vertebræ.

It is possible that Spitzbergen and Iceland Hares are of the same type as those of Greenland. None of these have come into my hands. The Bavarian, Swiss, Scottish, Irish and Siberian representatives of *timidus* are also likely to prove separable, at least into definable races, already named. From what is known of Linnæus at the time of writ-

ing his tenth edition of the System, it is most fitting that the Polar Hare of Southern Scandinavia should be made the type of the *timidus* group, the Swedish Hares being those which would most naturally embody and form the source of his original diagnosis.

The writer is now preparing a more compendious revision, with illustrations, of the New World representatives of the *Lepus timidus* group, which will probably appear in a future number of the Proceedings of the Academy of Natural Sciences of Philadelphia.

—SAMUEL N. RHOADS.

ENTOMOLOGY.[1]

On Certain Geophilidæ Described by Meinert.—The Chilopoda of the Museum of Comparative Zoology were studied by Dr. Meinert, and the results published in a paper entitled "*Myriapoda Musei Cantabrigensis.*"[2] Many new species were described, but as no figures were given, identification is not in all cases easy, although the descriptions are of considerable length. With reference to the Geophilidæ, at least, there are certain misleading statements and unfortunate omissions. During a recent visit to Cambridge I had the pleasure of a very brief examination of the types of several of Dr. Meinert's species, and some long-standing curiosity was satisfied.

Geophilus georgianus Meinert.

According to Dr. Meinert this species has but a single pleural pore. For some years past I have had specimens from the South which agreed well with the description of this species, but had two pores. As this character is a very constant one, my determination was not made with confidence. The type of *georgianus* has, however, two large pores on each side concealed under the last ventral plate, so that the anomaly is disposed of. The pores are similar in structure and location to those of *G. rubens.*

Geophilus cephalicus Wood.

The specimens described by Meinert, and previously by Wood as *cephalicus* belong to *G. rubens* Say. I have examined the type in the British Museum. It is the most common geophilid in the northeastern states.

Geophilus urbicus Meinert.

No ventral pores could be made out. The sterna are uneven and the whole animal is very hairy. The form of the body, the armature

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

[2] Proc. Am. Phil. Soc. XXI, pp. 161–233 (1885).

of the prehensorial legs and other strong similarities leave little doubt that this is a member of the genus Escaryus, as was conjectured when that genus was erected.[3] The anal legs were also strongly curved under as has been the case with all the specimens of Escaryus yet observed. That the differences enumerated between *E. phyllophilus* and *E. urbicus* can be maintained, is doubtful, for the Cambridge specimen is in rather poor condition, so that some of the characters ascribed by Dr. Meinert may easily prove to have been accidental.

Scolioplanes robustus Meinert.

The locality of this species was not known. I have collected what is evidently the same in central New York and southern Pennsylvania, and am unable to separate it from Sager's *Strigamia fulva*, the probable type of which I have seen in the Museum of the Academy of Natural Sciences at Philadelphia. The only difference between it and *bothriopus* and *robustus* seems to be that of size. The large specimens always show evidences of good living. The creatures are also constructed so as to be capable of considerable distention, besides being variable in size and number of legs, even in the same localities.

Scolioplanes parviceps Meinert.

The label in this bottle, probably in Meinert's handwriting is "*Scolioplanes parviceps* n. sp." The bottle also contains a label marked "*Strigamia bidens* Wood, N. A. loc.?" It is evidently to this label that Dr. Meinert refers when he says, (p. 226). "A specimen, which was said to be a type of Dr. Wood, was labeled '*Strigamia bidens* Wood.'" To thus rename a type specimen seems a remarkable proceeding, especially when the new name proposed has already been used in the same genus. Yet this is probably what Dr. Meinert proposed to do, for Mr. Henshaw kindly showed me a list of the collection, carefully made out in Dr. Meinert's handwriting, and in this the species is again given as new. That it did not so appear when the paper was printed, may have been the work of some American editor who knew of Wood's species and naturally supposed that the same was intended by Meinert.

Wood's *parviceps* is a Californian species, while *bidens* is found in the East. I have collected it in the vicinity of Washington. I had a specimen of *parviceps* at Cambridge with me to compare, but the difference was evident. There was no other specimen of *bidens* at hand, but the size, form of the body and other characters agree well with the eastern species.

[3] Proc. U. S. Nat. Museum XIII, p. 394 (1890).

Scolioplanes longicornis Meinert.

This species was looked upon by its author as the probable type of a new genus. The prehensorial claws are very long and slender, and the basal tooth very small. That it represents a new genus is well-nigh certain, but it would be idle to name it until drawings can be made.

Scolioplanes exul Meinert.

This is a large specimen with a strong general resemblance to large males of *fulvus* (*robustus*). The last pleuræ are without pores except close under the edge of the last ventral plate, where there is a large porose cavity. Anal legs with the claw minute, almost rudimentary, in this offering a strong contrast to the other American species known to me. The anal legs are also very robust, much stouter than a Californian specimen of *parviceps*.

Mecistocephalus breviceps Meinert.

The type specimen is minus the cephalic lamina and antennæ. There is another specimen labeled *breviceps*, but with no locality given. If the type was really collected at Nantucket the species must be very rare or local, for it seems not to have been found elsewhere.

Mecistocephalus heros Meinert.

It has been conjectured by Mr. Pocock that this species should be added to the long list of synonyms of *punctifrons*. I have never examined carefully authentic specimens of *punctifrons*, but the form of the prehensorial legs in the Cambridge specimen, especially the armature of the coxa is different from that of *Haase's* diagram of *punctifrons*. There is no distinct tooth, only a rounded prominence at the distal corner.

Himantarium indicum Meinert.

This specimen is in poor condition and has evidently been allowed to dry at some time in its history. The antennæ are distinctly attenuate. The ventral pores are in a posterior, transverse, subreniform area three or four times as broad as long. This area is scarcely depressed, but is quite definite. Pleural pores are not visible.

Himantarium tæniopse (Wood).

Ventral pores in a small, round, impressed, posterior area. No pleural pores visible, but they may be concealed under the very broad last ventral plate, as is the case in the following species.

Himantarium laticeps (Wood).

The ventral plates appear to be unusually long. The pores are located about two-thirds back, in broad, short, transverse areas. Three

large pleural pores, subconcealed. There seemed to be no specimen of *Himantarium insigne* Meinert in the collection.—O. F. Cook.

Life-history of Scale Insects.—In an excellent account of the Scale Insects affecting deciduous fruit trees Mr. L. O. Howard discusses[4] the life-history of the Coccidæ as follows: In respect to life history, the family Coccidæ, which includes all of the so-called scale insects, is very abnormal. The eggs are laid by the adult female either immediately beneath her own body or at its posterior extremity. Certain species do not lay eggs, but give birth to living young, as do the plant lice. This abnormal habit is not characteristic of any particular group of forms, but is found with individual species in one or more genera. The young on hatching from the eggs are active, six-legged, mite-like creatures which crawl rapidly away from the body of the mother, wander out upon the new and tender growth of the tree, and there settle, pushing their beaks through the outer tissue of the leaf or twig and feeding upon the sap. Even in this early stage the male insect can be distinguished from the female by certain differences in structure. As a general thing, the female casts its skin from three to five times before reaching the adult condition and beginning to lay eggs or give birth to young. With each successive molt the insect increases in size and becomes usually more convex in form. Its legs and antennæ become proportionately reduced, and its eyes become smaller and are finally lost. As a general thing, it is incapable of moving itself from the spot where it has fixed itself after the second molt, although certain species crawl throughout life. The adult female insect, then, is a motionless, degraded, wingless, and, for all practical purposes, legless and eyeless creature. In the armored scales she is absolutely legless and eyeless. The mouth parts, through which she derives nourishment, remain functional, and have enlarged from molt to molt. Her body becomes swollen with eggs or young, and as soon as these are laid or born she dies.

The life of the male differs radically from that of the female. Up to the second molt the life history is practically parallel in both sexes, but after this period the male larva transforms to a pupa, in which the organs of the perfectly developed, fledged insect become apparent. This change may be undergone within a cocoon or under a male scale. The adult male, which emerges from the pupa at about the time when the female becomes full grown, is an active and rather highly organized creature, with two broad, functional wings and long, vibrating antennæ.

[4] Yearbook U. S. Dept. Agr., 1894.

The legs are also long and stout. The hind wings are absent, and are replaced by rather long tubercles, to the end of each of which is articulated a strong bristle, hooked at the tip, the tip fitting into a pocket on the hind border of the wings. The eyes of the male insect are very large and strongly faceted. The mouth parts are entirely absent, their place being taken by supplementary eye spots. The function of the male insect is simply to fertilize the female, and it then dies. The number of generations annually among bark lice differs so widely with different forms that no general statement can be made.

EMBRYOLOGY.[1]

The development of Isopods.—Last Winter when M. Louis Roule published a long paper in French on the development of an Isopod, *Porcellio scaber* Leach, it seemed advisable to present a rather full abstract in this magazine, for the benefit of those readers who would not see the original or who did not read French. That abstract appeared in February and contained, besides the descriptive account of the embryology, some interesting conclusions based on these results.

In the May number of the Journal of Morphology Dr. J. Playfair McMurrich publishes a long paper, illustrated with excellent figures, which is not at all reconcilable with M. Roule's views. It must be remembered, in comparing the two papers, that M. Roule studied a single species of Isopod, that he gives rather diagrammatic figures, and that his description of the segmentation, on which apparently the whole fabric rests, is of a very general nature.

Dr. McMurrich took up the work in 1890, hoping to make out the cytogeny of a Crustacean as Whitman had done for Clepsine, and as E. B. Wilson has later done for Nereis and other forms. This author's results rest then on a thorough study of the segmentation, and as he did not confine his attention to one form, but observed and figured the segmentation and early differentation in a number of Isopods, the paper is of especial interest.

The forms studied were *Jæra marina* Möbius (1873); *Asellus communis* Say; *Porcellio scaber*; *Armadillidium vulgare*; with some observations on *Cymothoa* and *Ligia*.

The segmentation is centrolecithal. The nucleus of the unsegmented ovum lies in a central mass of protoplasm surrounded by yolk, and

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts, reviews and preliminary notes may be sent.

from the central protoplasm a network extends out through the yolk to a peripheral layer. It is possible to determine the second plane of division as that of the long axis of the embryo as has been shown to be the case in Nereis, Crepidula, and Umbrella. In discussing the segmentation, the author uses the term cell as a convenient one for the nucleated bodies of protoplasm which appear on division in the yolk, but he insists on the syncitial nature of the ovum to a late period.

The cleavage is apparently a spiral one, and results in what may be spoken of as a blastula stage, in which considerable differentiation has taken place. From the eight cell stage, especially in Jæra, it is possible to trace the history of different areas of this blastula to certain well marked cells. For instance, in Jæra, a cell at the posterior pole gives rise to the future Vitellophags, three cells immediately encircling it are the ancestors of the mesendoderm, while the ectoderm arises from the remaining four anterior cells. There are some interesting variations in this history in the forms studied, though the end result is practically the same. The author concludes with E. B. Wilson that, "cells having precisely the same origin in the cleavage, occupying the same position in the embryo, and placed under the same mechanical conditions may nevertheless differ fundamentally in morphological significance."

In connection with the segmentation Dr. McMurrich thinks that the existence of a syncitium up to so late a period in differentiation is of special interest in relation to the current disscusion of the cell-theory. The question is asked, "are we to believe that there is no continuity in Lucifer, between the blastomeres, notwithstanding that in all probability there was continuity in the ova of its ancestors?" In *Peripatus capensis* there is an approach to holoblastic cleavage associated with less yolk and still a syncitium results. This is regarded as supporting "the supposition that, even in such cases as Lucifer, there may be also a continuity of protoplasm, the separation into distinct spherules being only apparent."

It should be remembered that, however, plausible this argument is, of course the fact of continuity between the blastomeres of Lucifer or of other holoblastic ova still remains to be proven by direct observation.

The conclusion that "the existence of a syncitium is no bar to a certain amount of differentiation," certainly seems justified from the facts described for Jæra. Continuing this subject, such a syncitium is compared with the differentiation in certain protozoa, and a peculiar phenomenon in Porcellio is considered, where there is a precocious segregation of a portion of the cytoplasm which is to take part in the

formation of the blastoderm. This segregation is said to take place, "not in accordance with any previous location of a nucleus, but independently." Dr. McMurrich thinks that "this phenomenon seems to demonstrate that cytoplasmic differentiation may occur *independently of definite nuclear influence.*" He immediately adds, however, that "he, of course does not mean to assert that the nuclei may not possess a *coördinating or even a trophic action upon the cytoplasm,* but that they are directly responsible for the segregation or concentration seems to him an unwarranted assumption."

It is difficult to understand just what is meant by these statements. The remarkable concentration of the peripheral protoplasm of the ovum of Porcellio toward the definitive ventral side, independently of any previous location of nuclei, is notewothy. Does it, however, "demonstrate cytoplasmic *differentiation* independent of definite nulcear influence?" Can this *movement* of protoplasm, even toward a definite point, be correctly spoken of as *differentiation* and compared with the specialization in the cytoplasm of certain protozoa? Having in mind the condition of the ovum when this phenomenon takes place, is it not possible that the movement may be the result of nuclear influences from the center, acting through the central network on the periphal protoplasm?

Again, if the phenomenon demonstrates cytoplasmic differentiation *independent* of definite nuclear influence, why does the author add that, "he does not mean to assert that the nuclei may not possess a *coördinating* action upon the cytoplasm?" There seems to be a contradiction in these two statements, which may destroy the force of the argument. It should also be remembered that in Jæra, and in the other Isopods studied, there is a contraction of the blastoderm cells toward the ventral surface. Here the nuclei, as well as the cytoplasm, of the blastoderm are evidently directly involved. Perhaps the precocious segregation of cytoplasm ventrally in Porcellis is but an early appearance of this process. If it be admitted that the nuclei possess coördinating influences on the cytoplasm, how can it be claimed that in the case of the highly differentiated protozoa such influences were not active during the differentiation?

Another point discussed is the extent of external influences and their action, in holoblastic and in centrolecithal ova like those of Jæra. The conclusion reached from a review of the facts of segmentation is that "the cleavage form of Jæra is determined entirely by intrinsic conditions." The phenomena of segmentation "leave us no choice but to refer the vis essentialis which determines the direction of the Karyokinetic

spindle, and therefore, the cleavage form of Jæra, to the constitutional peculiarity of the ovum." "Holoblastic ova, the author believes, can not be excluded from the action of external forces, but the presumption is allowable, for several reasons, that even in these intrinsic forces are important." The assumption must consequently be made "that intrinsic forces reside in all ova, though they may be overshadowed by external influences in some cases."

It is important to examine the assumption which forms the foundation of this argument. The author quotes E. B. Wilson's conclusion that "cleavage forms are not determined by mechanical conditions alone," and assumes that by "mechanical conditions," Wilson means conditions extrinsic to the ovum. This can hardly be so, for it is necessary to include among the "mechanical conditions" influencing the cleavage of an ovum like that of Jæra, the presence in the cytoplasm of a great accumulation of food-yolk, (excessive in quantity when compared with that in holoblastic or meroblastic ova). It is true that this mass is within the ovum, and in so far "intrinsic", but its action is usually looked on as that of a foreign body, so to speak, which modifies and obscures the primitive phenomona of cleavage and differentiation as seen in holoblastic ova. Hence it is important to remember that Dr. McMurrich, in maintaining that "the cleavage form of Jæra is determined entirely by intrinsic conditions," must include the action of the nutritive mass. This would seem to weaken materially the position that extrinsic influences, (in the generally accepted sense as extrinsic to active cytoplasm), are excluded from action on the spindles of centrolecithal ova. The confusion seems to lie in the use of the word intrinsic to include, in the case of the ovum of Jæra, both inherent properties of the protoplasm, and secondary forces due to the presence of a body of nutritive material which is morphologically not a part of the protoplasm. Dr. McMurrich's conclusion, that "instrinsic forces reside in all ova", or preferably, as E. B. Wilson has just it, "cleavage forms are not determined by mechanical conditions alone," will probably be accepted as truth by most observers. However, I can not see that he has shown that "in Jæra we have practically a demonstration of the correctness of this view" of a more convincing character than is exhibited by holoblastic ova.

"The cleavage form of Jæra, is said to be, determined entirely by intrinsic conditions." A conclusion from which Dr. McMurrich sees no escape, after a review of the changes of position in the yolk assumed by the nuclei during segmentation. The Karyokinetic spindles then are regarded as entirely beyond the influence of forces external to the

ovum in such eggs as those of Jæra, and their direction, with consequently the cleavage form, is due without other alternative entirely to the constitutional peculiarity of the ovum. After carefully considering the evidence presented by Jæra and similar centrolecithal eggs, the assumption does not seem warranted, that they are any more removed from the influences of external forces, than are holoblastic ova. It may be true that it is difficult to *understand* how forces external to a centrolecithal ovum may affect the spindles within it, but many will find the same difficulty in the case of holoblastic ova. Does the great increase of yolk in a centrolecithal ovum remove the spindles from the action of the external world? I, for one, can not see that this necessarily follows, and hence do not see that the condition of segmentation in Jæra leaves us no escape from the conclusion that its cleavage form is determined entirely by intrinsic conditions.

Returning to the description of the embryo, it will be remembered that the germ-layers are already distinguishable in a blastula stage on the surface of the yolk in Jæra, and somewhat less distinctly in the other forms. Now the blastoderm cells gradually concentrate towards the ventral surface of the egg. This results in the mesendoderm and vitellophag cells being crowded beneath the surface in the form of a solid plug, and in the ectoderm of the ventral surface marking out a somewhat triangular area, the base of which lies anteriorly while the apex is posterior. This area is the Nauplius region. The rudiments of the eyes are placed anteriorly at the angles of the base, the appendages appear later along the sides, while the blastoporic plug of mesendodermal cells lies just under the posterior apical end. In a most interesting discussion of the formation of the germs layers in the Crustacea, the author concludes that the primitive Crustacea probably passed through a blastula stage which was filled with yolk, and in which a plug of cells migrated into the yolk to be later differentiated into mesoderm and endoderm. This is the condition exhibited by the Phyllopods (Samassa, 1893 and Bauer, 1892). Jæra, the Decapods and especially Lucifer are examples of precocious differentiation of the germ layers. The entire mesendoderm of Crustacea has a blastoporic origin, and is not (except in Decapods, where there are secondary phenomena) formed by delamination of extra-blastoporic region. The under layer of the latter regions is formed by a migration of cells from the blastoporic plug. In Armadillidium this is especially well made out. An interesting question is raised in regard to the mesenteron of Astacus. Dr. McMurrich suggests the probability that the yolk-pyramids do not form it, but eventually form mesodermic tissues, while

the mesenteron is really formed by cells of the entodermic plates. This interpretation would be more in line with what is known of other Crustacea. Some of the most interesting observations and conclusions of the paper are those concerning the development of the metanaupliar regions of the embryo. It is a remarkable fact that the Naupliar and more posterior metanaupliar regions are very sharply distinguished by differents methods of growth. Dr. Patten was the first to call attention to the fact of teloblastic growth in the ectoderm and mesoderm of Cymothoa. Dr. McMurrich has gone further, and in his comparative study, has made out in detail the character and limits of this method of growth in Isopods. While the Naupliar is formed as described, the metanaupliar regions, are the result of teloblastic growth in ectoderm and mesoderm, just as the metatrochophoral regions of Polygordius are due to a similar process. The author is inclined to regard these two instances of teloblastic growth as acquired independently. He thinks that in the Isopod "the development points back to a period where a free-swimming Nauplius occurred in the development of the ancestors of the group, the egg embryo being a nauplius." At such a time the metanaupliar regions were developed after hatching. Now, however, this posterior region is developed in Isopods before hatching, but it still retains the peculiar teloblastic method of development, and is sharply distinguishable from the Naupliar area.

There is unfortunately not space to describe this remarkable process. It is interesting to note, however, that, while the ectoderm of the metanaupliar regions arises from the successive divisions of a row of ectodermal teloblasts, the rythm of these divisions is not the same as that of the row of mesodermal teloblasts which lies beneath. The mesodermal teloblasts divide just 16 times giving rise to 16 transverse rows of mesoderm cells, "each of which rows is equivalent to a segment," as is proved on the appearance of appendages. The ectodermal teloblasts divide twice as many times.

Though these are the main points of the paper, a number of important observations and conclusions have been necessarily crowded out of this review. For instance, I have not touched on the processes of impregnation, the formation of membranes, the details of segmentation and differentiation, the formation of the digestive tract, the history of the vitellophages, or the development of certain organs.

—H. McE. Knower.

PSYCHOLOGY.

Consciousness and Evolution.—The quotation by Professor Cattell in SCIENCE, July 26, of Professor Cope's table (from the *Monist*, July, 1895) shows that he was equally struck by it with myself. Prof. Cope gives in this table certain positions on points of development, in two contrasted columns, as he conceives them to be held by the two camps of naturalists divided in regard to inheritance into Preformists and the advocates of Epigenesis. The peculiarity of the Epigenesis column is that it includes certain positions regarding consciousness, while the Preformist column has nothing to say about consciousness. Being struck with this I wrote to Professor Cope—the more because the position ascribed to consciousness seemed to be the same, in the main, as that which I myself have recently developed from a psychological point of view in my work on *Mental Development* (Macmillan & Co.). I learn from him that the table[1] is not new; but was published in the the 'annual volume of the Brooklyn Ethical Society in 1891;' and the view which it embodies is given in the chapter on 'Consciousness in Evolution;' in his *Origin of the Fittest* (Appletons, 1887).

Apart from the questions of novelty in Professor Cope's positions—and that Mr. Cattell and I should both have supposed them so can only show that we had before read hastily; I myself never looked into Professor Cope's book until now—I wish to point out that the placing of consciousness, as a factor in the evolution process, exclusively in the Epigenesis column, appears quite unjustified. It is not a question, as Mr. Cattell seems to intimate in his note referred to in SCIENCE, July 26, of a causal interchange between body and mind. I do not suppose that any naturalist would hold to an injection of energy in any form into the natural processes by consciousness; though, of course, Professor Cope himself can say whether such a construction is true in his case. The psychologists are, as Mr. Cattell remarks, about done with a view like that. The question at issue when we ask whether consciousness has had a part in the evolutionary process is, I think, as to whether we say that the presence of consciousness—say in the shape of sensations of pleasure and pain—with its nervous or organic correlative processes, has been an essential factor in evolution; and if so, further,

[1] This table is given in the issue of SCIENCE for July 26, p. 100. The three points from it which are taken up now are cited below.

whether its importance is because it is through the consciousness aspect of it that the organic aspect gets in its work. Or, to take a higher form of consciousness, does the memory of an object as having given pleasure help an organism to get that object a second time? This may be true, although it is only the physical basis of memory in the brain that has a causal relation to the other organic processes of the animal.

Conceiving of the function of consciousness, therefore, as in any case not a *deus ex machina,* the question I wish to raise is whether it can have an essential place in the development process as the Preformists construe that process. Professor Cope believes not. His reasons are to appear fully in his proposed book. I believe that the place of consciousness may be the same—and may be the essential place that Mr. Cope gives it in his left-hand column and which I give it in my *Mental Development*—on the Preformist view. I have argued briefly for this indifference to the particular theory one holds of heredity, in my book (Chap. VII.), reserving for a further occasion certain arguments in detail based upon the theory of the individual's personal relation to his social environment. The main point involved, however, may be briefly indicated now, although, for the details of the social influences appealed to, I must again refer to my book (Chaps. on 'Suggestion' and 'Emotion').

I have there traced out in some detail what other writers also have lately set in evidence, *i. e.*, that in the child's personal development, his ontogenesis, his life history, he makes a very faithful reproduction of his social conditions. He is, from childhood up, excessively receptive to social suggestion; his entire learning is a process of conforming to social patterns. The essential to this, in his heredity, is excessive instability, cerebral balance and equilibrium, a readiness to overflow into the new channels which his social environment dictates. He has to learn everything for himself, and in order to do this he must begin in a state of great plasticity and mobility. Now, my point, but briefly, is that these social lessons which he learns for himself take the place largely of the heredity of particular paternal acquisitions. The father must have been plastic to learn, and this plasticity is, as far as evidence goes, the nervous condition of acute consciousness; the father then learned, through his consciousness, from his social environment. The child does the same. What he inherits is nervous plasticity and the consciousness. He learns particular acts for himself; and what he learns is, in its main line, what his father learned. So he is just as well off, the child of Preformism, as if he had been the heir of the particular lessons of his father's past. I have called this process 'Social Hered-

ity,' since the child really inherits the details; but he inherits them from society by this process of social growth, rather than by direct natural inheritance.

To show this in a sketchy way, I may take the last three points which Professor Cope makes under the Epigenesis column, the points which involve consciousness, and show how I think they may still be true to the Preformist if he avail himself of the resource offered by 'Social Heredity.'

I do this rather for convenience than with any wish to controvert Professor Cope; and it may well be that his later statements may show that even this amount of reference to him is not justified.

1. (5 of Cope's table). "Movements of the organism are caused or directed by sensation and other conscious states."

The point at issue here between the advocate of Epigenesis and the Preformist would be whether it is necessary that the child should inherit any of the particular conscious states, or their special nervous dispositions, which the parent learned in his lifetime, in order to secure through them the performance of the same actions by the child. I should say, no; and for the reason—additional to the usual arguments of the Preformists—that 'Social Heredity' will secure the same result. All we have to have in the child is the high consciousness represented by the tendency to imitate the parent or to absorb social copies, and the general law now recognized by psychologists under the name of Dynamogenesis—*i. e.*, that the thought of a movement tends to discharge motor energy into the channels as near as may be to those necessary for that movement.[2] Given these two elements of endowment in the child, and he can learn anything that his father did, without inheriting any particular acts learned by the parent. And we must in any case give the child this much; for the principle of Dynamogenesis is a fundamental law in all organisms, and the tendency to take in external 'copies' by imitation, etc., is present in all social animals, as a matter of fact.

The only hindrance that I see to the child's learning everything that his life in society requires would be just the thing that the advocates of Epigenesis argue for—the inheritance of acquired characters. For such inheritance would tend so to bind up the child's nervous substance in fixed forms that he would have less or possibly no unstable substance left to learn anything with. So, in fact, it is with the animals in which instinct is largely developed; they have no power to learn any thing new, just because their nervous systems are not in the mobile

[2] Both of these requirements are worked out in detail in my book.

condition represented by high consciousness. They have instinct and little else. Now, I think the Preformist can account for instinct also, but that is beside the point; what I wish to say now is that, if Epigenesis were true, we should all be, to the extent to which both parents do the same acts (as, for example, speech) in the condition of the creatures who do only certain things and do them by instinct. I should like to ask of the Neo-Lamarckian: What is it that is peculiar about the strain of heredity of certain creatures that they should be so remarkably endowed with instincts? Must he not say in some form that the nervous substance of these creatures has been 'set' in the creatures' ancestors? But the question of instinct is touched upon under the next point.

2. (6 of Cope's table). "Habitual movements are derived from conscious experience." This may mean movements habitual to the individual or to the species in question. If it refers to the individual it may be true on either doctrine, provided we once get the child started on the movement—the point discussed under the preceding head. If, on the other hand, habitual movements mean race movements, we raise the question of race habits, best typified in instinct. I agree with Mr. Cope that most race habits are due to conscious function in the first place; and making that our supposition, again we ask: Can one who believes it still be a Preformist? I should again say that he could. The problem set to the Preformist would not in this case differ from that which he has to solve in accounting for development generally: it would not be altered by the postulate that consciousness is present in the individual. He can say that consciousness is a variation, and what the individual does by it is 'preformed' in this variation. And then what later generations do through their consciousness is all preformed in the variations which they constitute on the earlier variations. In other words, I do not see that the case is made any harder for the Preformist by our postulate that consciousness with its nervous correlate is a real agent. And I think we may go further and say that the case is easier for him when we take into account the phenomena of Social Heredity. In children, for example, there are variations in their mobility, plasticity, etc.; in short, in the ease of operation of Social Heredity as seen in the acquisition of particular functions. Children are notoriously different in their aptitudes for acquiring speech, for example; some learn faster, better, and more. Let us say that this is true in animal communities generally; then these most plastic individuals will be preserved to do the advantageous things for which their variations show them to be the most fit. And the next generation will

show an emphasis of just this direction in its variations. So the fact of Social Heredity—the fact of acute use of consciousness in ontogeny —becomes an element in phylogeny, also, even on the Preformist theory.

Besides, when we remember that the permanence of a habit learned by one individual is largely conditioned by the learning of the same habits of others (notably of the opposite sex) in the same environment, we see that an enormous premium must have been put on variations of a social kind—those which brought different individuals into some kind of joint action or coöperation. Wherever this appeared, not only would habits be maintained, but new variations, having all the force of double hereditary tendency, might also be expected. But consciousness is, of course, the prime variation through which coöperation is secured. All of which means, if I am right, that the rise of consciousness is of direct help to the Preformist in accounting for race habits—notably those known as gregarious, coöperative, social.

3. (7 of Cope's table). "The rational mind is developed by experience, through memory and classification." This, too, I accept, provided the term 'classification' has a meaning that psychologists agree to. So the question is again: Can the higher mental functions be evolved from the lower without calling in Epigenesis? I think so. Here it seems to me that the fact of Social Heredity is the main and controlling consideration. It is notorious how meagre the evidence is that a son inherits or has the peculiar mental traits of parents beyond those traits contained in the parents' own heredity. Galton has shown how rare a thing it is for artistic, literary or other marked talent to descend to the second generation. Instead, we find such exhibitions showing themselves in many individuals at about the same time, in the same communities, and under the same social conditions, etc. Groups of artists, musicians, literary men, appear, as it were, as social outbursts. The presuppositions of genius—dark as the subject is—seem to be great power ef learning or absorbing, marked gifts or proclivities of a personal kind which are not directly inherited but fall under the head of sports or variations, and then a social environment of high level in the direction of these sports. The details of the individual development, inside of the general proclivity which he has, are determined by his social environment, not by his natural heredity. And I think the phylogenetic origin of the higher mental functions, thought, self-consciousness, etc., must have been similar. I have devoted space to a detailed account of the social factors involved in the evolution of these higher faculties in my book.

I fail to see any great amount of truth in the claims of Mr. Spencer that intellectual progress in the race requires the Epigenesis view. The level of culture in a community seems to be about as fixed a thing as moral qualities are capable of being; much more so than the level of individual endowment. This latter seems to be capricious or variable, while the former moves by a regular movement and with a massive front. It would seem, therefore, that intellectual and moral progress is gradual improvement, through improved relationships on the part of the individuals to one another; a matter of social accommodation, rather than of natural inheritance alone, on the part of individuals. It is only a rare individual whose heredity enables him to break through the lines of social tissue and imprint his personality upon the social movement. And in that case the only explanation of him is that he is a variation, not that he inherited his intellectual or moral power Furthermore, I think the actual growth of the individual in intellectual stature and moral attainment can be traced in the main to certain of the elements of his social *milieu*, allowing always a balance of variation in the direction in which he finally excels.

So strong does the case seem for the Social Heredity view in this matter of intellectual and moral progress that I may suggest an hypothesis which may not stand in court, but which I find interesting. May not the rise of the social life be justified from the point of view of a second utility in addition to that of its utility in the struggle for existence as ordinarily understood; the second utility, *i. e.*, of giving to each generation the attainments of the past which natural inheritance is inadequate to transmit? Whether we admit Epigenesis or confine ourselves to Preformism, I suppose we have to accept Mr. Galton's law of Regression and Weismann's principle of Panmixia in some shape. Now when social life begins we find the beginning of the artificial selection of the unfit; and so these negative principles begin to work directly in the teeth of progress, as many writers on social themes have recently made clear. This being the case, some other resource is necessary besides natural inheritance. On my hypothesis it is found in the common or social standards of attainment which the individual is fitted to grow up to and to which he is compelled to submit. This secures progress tn two ways: First, by making the individual learn what the race has learned, thus preventing social retrogression, in any case; and seecond, by putting a direct premium on variations which are socially available.

Under this general conception we may bring the biological phenomena of infancy, with all their evolutionary significance: the great

plasticity of the mammal infant as opposed to the highly developed instinctive equipment of other young; the maternal care, instruction and example during the period of helplessness, and the very gradual attainment of the activities of self-maintenance in conditions in which social activities are absolutely essential. All this stock of the development theory is available to confirm this view.

And to finish where we began, all this is through that wonderful engine of development, consciousness. For consciousness is the avenue of all social influences.—J. MARK BALDWIN, Princeton.

The preceding communication from Prof. Baldwin is copied from Science of August 23, 1895. It is reprinted in order to render intelligible a review of it which I propose to publish in the next number of the NATURALIST.—E. D. COPE.

ANTHROPOLOGY.[1]

Mercer's Cave Explorations in Yucatan.[2]—This a handsomely illustrated volume which describes in detail the researches made by the Corwith Expedition to Yucatan, under the direction of Mr. H. C. Mercer of the University of Pennsylvania. The object of the expedition was to search for the remains of prehistoric man in the cave deposits, and to learn who were the predecessors or ancestors of the peoples whose civilization is attested by the remarkable ruins which are such a conspicuous feature of that country. Explorations of this kind made in Europe have achieved such important results to archeology, that every research in America must be watched with great interest. As a summary of his work, Mr. Mercer remarks:

"The intervening two months seemed a long time; nor was it easy to realize that, after all, the area gone over had not exceeded one hundred miles in length by ten in breadth. Twenty-nine caves had been visited in sixty days, of which ten had been excavated. Thirteen had archeological significance. Six had yielded valuable, and three, decisive results.

"We had seen but little of the ruins. We had not passed southward over the boundary line into the great wilderness, whence fables of lost cities reach the traveller's ear. Our continued study of an un-

[1] This department is edited by H. C. Mercer, University of Pennsylvania.

[2] The Hill Caves of Yucatan: A Search for the Evidence of Man's Antiquity in Central America; being an account of the Corwith Expedition of the Department of Archeology and Paleontology of the University of Pennsylvania, by Henry C. Mercer. J. B. Lippincott & Co. Philadelphia, 1896. 8vo., pp. 183.

derground layer of human refuse substantially the same in all the caves, instructive as it was, had taught us but little of details. Evidently a wide range of tools and implements had not been left, lost or broken in the subterranean rooms. We did not find, and did not expect to find, that the water producing underground chambers had been used as burying places. Neither were they dwellings, but rather temporary halting spots, which, but for the water supply, would probably have shown fewer human traces than do the caves of the United States. Human bones scattered in the rubbish indicated that the old inhabitants of Yucatan practiced cannibalism. Beyond that, the traces of pre-Columbian cookery at the underground sites referred to an ancient cave visitor, who was rather an agriculturist than a hunter, and who (unless the dog found at Sabaka be an exception) possessed no domestic animals.

"We had learned little of stone chipping, and had found in the scanty list of stone blades but one imperfect point that might have served for an arrowhead. The secret of stone carving we had failed to discover, and though the whole mystery had seemed within our grasp at Oxkintok, we had to rest content with proving that the chiselling of the ruins could not have been done with chips of the parent block or round hammer stones. We had found no copper, or gold, or silver, no jade, no gums, no preserved grains, no cloth, no apparatus for weaving, and had discovered no pipe, and learned nothing of pre-Columbian smoking or tobacco.

"A close examination of the potsherds showed a ware mixed with powdered limestone that reacted strongly under acid on the fractures. A smooth red make, strong, wellbaked, and symmetrical, and whose dull polished surface resisted the action of nitric acid, was abundant, while a very few fragments were decorated with brightly colored designs, though their polish, after the manner of varnish, yielded readily to the acid test. Many, though better baked than the ware of the Delaware Indians, were coarse. A very common hard variety had been striped with brown lines on a white or bluish background. But there was nothing brilliant or striking about these fragments of dishes, cooking pots, or water jars. Few were ornamented, and only two or three highly so. None were marked with hieroglyphs. Nevertheless, a variety of tones, colors, and polish struck the eye when many sherds were laid side by side and brushed.

"But results more important than these had rewarded our close examination of the position and contents of the human rubbish heap everywhere present in the caves. Though this layer was the only cul-

PLATE VI.

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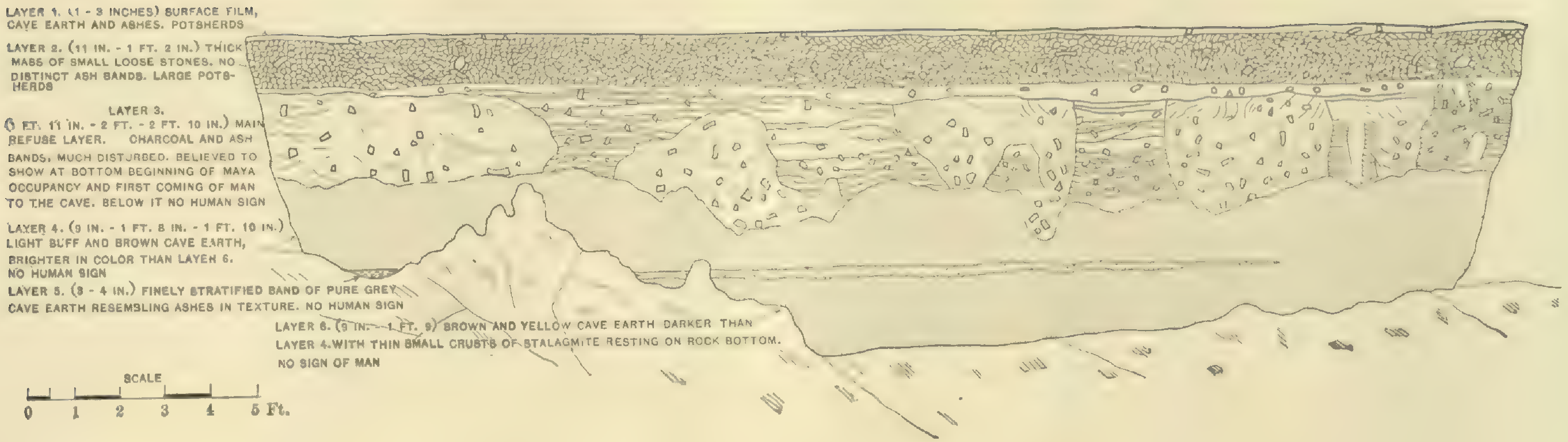
1. Cave of Sayab Actun, interior. 2. Section of Cave of Actun Xmak.

PLATE VII.



Section of Cave of Loltun.

ture layer, our digging had fairly proved at Oxkintok, Loltun and Sabaka, and though we had often failed to reach rock bottom at other caverns, there was nowhere ground for supposing that deeper digging or blasting would have upset our inference. An earlier people visiting Yucatan under its present topographical conditions must needs have left their trace in the caves, and because the undisturbed earth beneath the culture layer discovered, always failed to show trace of any deeper, older or more primitive human visitor, the conclusion was that no such earlier people had seen the region while its stony hills, its torrid plain, and its damp caves were as they now are."

The evidence secured by Mr. Mercer justifies this conclusion so far as it goes. To prove that a human population existed in Yucatan prior to that whose remains were actually found, it will be necessary to discover another series of deposits inside or out of an older type of caves. No such caves were found, and while it cannot be asserted that such will not be found, it is evident that they must be very rare if existing in the region explored. The case of Yucatan may prove to be similar to that of the United States, where I have shown on paleontologic grounds,[3] that cave deposits of two different ages exist. The remains of vertebrate life found in the caves of Yucatan explored by Mr. Mercer, are those of the existing fauna of the country, and the deposits correspond, therefore, with those of the second (postchamplain) age of the northern caves. Caves of prechamplain age are rare in the United States, as shown by Mr. Mercer's earlier researches, having been probably removed by the action of water during the Champlain submergence. That such a submergence may have also taken place in Yucatan is indicated by the recent researches of Spencer; but if so, a cleaner sweep of them was made than was the case in North America.

Among the remains of animals which were discovered, those of the horse occurred in two caves, and the dog in one. It is probable they both belong to the domesticated species.

I append some examples of the very admirable illustrations with which the book abounds.

Apart from its scientific value, this book will interest the general reader for various reasons. It is written in a pleasant style, and many side lights are thrown on the characters of the country and people. That the exploration was not without the element of danger is shown by the tragic death of one of the natives; while the sufferings of the

[3] American Naturalist, 1895, p. 598.

party from heat and insects show that none but hardy explorers could undertake such labor. We recommend the book as an admirable ex-

Interior of grand rotunda of Cave of Actun Benado.

ample of the combination of utility with adventure which characterizes scientific research in the wilds.—E. D. COPE.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

Academy of Science of St. Louis.—President Gray in the chair and twenty-two other persons present, Mr. Trelease exhibited several specimens, about three feet square, of a curious silk tapestry, taken from the ceiling of a corn-storing loft in San Luis Potosi, Mexico, by Dr. Francis Eschauzier, stating that he was informed that the larger specimen had been cut from a continuous sheet over twenty yards wide and about four times as long. The specimens, of a nearly white color, and of much the appearance and feeling of a soft tanned piece of sheepskin, were shown to be composed of myriads of fine silken threads, crossing and recrossing at every conceivable angle, and so producing a seemingly homogeneous texture. Although specimens of the creatures by which they are produced had not been secured, it was stated that there

was no doubt that these tapestries are the work of lepidopterous larvæ which feed upon grain, the presumption being that they are made by the larvæ of what has been called the Mediterranean Grain or Flour Moth (*Ephestia kühniella*). The speaker briefly reviewed the history of this insect and its injuriousness in various parts of the world, and quoted from a report of Dr. Bryce, showing that in Canada, where it became established in 1889, "a large warehouse, some 25 feet wide, 75 feet long, and four stories high, became literally alive with moths in the short course of six months."—WILLIAM TRELEASE.

Boston Society of Natural History.—February 5th.—The following paper was read: Mr. Herbert Lyon Jones, "Biological adaptations of desert plants to their surroundings.—SAMUEL HENSHAW, *Secretary.*

Nova Scotian Institute of Science.—13th of January.—The following papers were read: "Notes on the Superficial Geology of Kings County, Nova Scotia," by Prof. A. E. Coldwell, M. A., Acadia College. "A Note on Newton's Third Law of Motion," by Prof. Mac Gregor, D. Sc., F. R. SS. E. & C., Dalhousie College.—HARRY PIER, *Secretary.*

New York Academy of Science, Biological Section.—January 13th, 1896.—The papers presented were: G. S. Huntington on "*The Visceral Anatomy of the Edentates.*" The characters of the brain, alimentary, respiratory and genito-urinary tracts were especially considered. The following forms were discussed: *Myrmecophaga jubata, Tamandua bivittata, Arctopithecus didactylus, Dasypus sexcinctus, Tatusia novemcincta, Manis longicaudata.* In the brain characters the following features were considered;—the transverse frontal sulcus, the great longitudinal fissure, and the absence of a distinct Sylvian fissure. In the alimentary tract the Sloths are to be sharply separated from the remaining groups, the stomach structure with its pyloric gizzard notably aberrant: the ileo-colic junction is traced throughout the edentates in a well marked series of transitional forms.

O. S. Strong, "*On the Use of Formalin in Injecting Media.*" The paper made especial note of the advantages possessed by this preservative in injecting the brain *in situ.* Formalin (40 per cent formaldehyde) diluted with an equal volume of water is injected into the cephalic vessels until it runs from the cut jugulars. After a few minutes the same quantity is again injected and once or twice again after an elapse of fifteen to twenty minutes. The brain is then removed and will be found to be completely fixed throughout. The swelling usually

noticed in formalin hardened brains does not appear to take place when this method is employed. Besides the many general advantage of fixing brains by injection, formalin has the especially merit of giving them the best consistency for macroscopic work, and further such brains are available subsequently for the Golgi and Weigert methods as well as, possibly, for cytological methods. Formalin has also the advantage that it can be used, as above, stronger than is necessary for fixation and thus allowance made for its dilution when permeating the tissue. When only the Golgi method is to be used, an equal volume of a 10 per cent solution of potassium bichromate may be added to the formalin instead of water. Pieces can be subsequently removed, hardened further in formalin-bichromate and impregnated with silver.

Bashford Dean, "*On the Supposed Kinship of the Paleospondylus.*" A favorably preserved specimen of this interesting fossil, received by the writer from Wm. T. Kinnear of Forss, Scotland, appears to warrant the belief that this lamprey-like form was possessed fins, a character decidedly adverse to the now widely accepted view of Marsipobranchian affinities. The structure referred to consists of a series of transversely directed rays, arising from the region of the postoccipital plates of Traquair. From this peculiar character, as well as from many unlamprey-like features of the fossil, it would appear accordingly that the kinship of the Paleospondylus is as yet by no means definitely determined.—C. L. BRISTOL, *Secretary.*

Nebraska Academy of Sciences.—The following program of papers was presented. *First Session—Thursday, Jan.* 2, 1896. "America the Primitive Home of Civilization," H. S. Clason; "The Home of the Buffalo Grass," Dr. C. E. Bessey; "Early Rainfall Records in Nebraska," G. D. Swezey; "The Volcanic Ashes of Nebraska," Dr. E. H. Barbour. *Second Session—Friday, Jan.* 3.—"The Relative Importance of Economic Fungi, East and West," F. W. Card; "Animal Parasites of Nebraska," Dr. H. B. Ward; "Diatomaceous Deposits of Nebraska," Dr. E. H. Barbour; "Some Fossil Diatoms from Nebraska," C. J. Elmore; "Wind Velocities in Nebraska," G. A. Loveland; "Report of Progress on the Study of Dæmonelix," Dr. E. H. Barbour; "Origin of the Present Flora of Nebraska," Dr. C. E. Bessey.

SCIENTIFIC NEWS.

Huxley Memorial.—Since the first meeting of the General Committee on November 27, which was fully reported by the Press, two meetings of the Executive Committee have been held.

At the first of these, at which Lord Shand accepted the office of Chairman, it was reported that a number of foreigners of eminence had expressed a wish to be associated with the proposal to commemorate Mr. Huxley's distinguished services to humanity. It was resolved, in the first instance, to invite subscriptions from the members of the General Committee.

At the second meeting, held on December 18, it was reported that the subscriptions, which at the General Meeting had amounted to £557, had been increased to about £1,400, and it was resolved that a wider appeal for subscriptions should now be made to the friends and admirers of Mr. Huxley amongst the general public. The sum subscribed now exceeds £1,500.

The Honorary Secretary stated that in America Committees were in the course of being formed to promote the realization of an adequate fund.

The Committee resolved to communicate, by means of a sub-committee of their number, with Mr. Onslow Ford, R. A., who had the advantage of being well acquainted with Mr. Huxley, in reference to the statue, which it is proposed should be erected beside those of Darwin and Owen in the Natural History Museum, South Kensington.

The extent to which the Committee may be able to carry out the other intended objects of founding exhibitions, scholarships, and medals for biological research and lectureships, and possibly in assisting the republication of Mr. Huxley's scientific works, will, of course, depend on the subscriptions which may now be received.

Meehans' Monthly is a magazine for the lovers of gardening; and covers the whole field of general intelligence in so far as it may have the remotest bearing on the chief topics it sets out to advance. For instance, a beautiful Prang colored plate of some wild flower is given every month, with a description which illustrates the whole ground of classical history that has any bearing on the topic. Information on the most diversified topics abound. Corn from Indian mounds will not grow—swamps that are real swamps are among the healthiest of localities. There is no sickness in the great dismal swamp of Virginia.

Elderberry root is found to be a deadly poison. Foul water is pronounced to be a self-purifier, because bacteria eat out vegetable matter and then die of starvation. The hickory and the chestnut are proven cousin—Germans. Weeds are useful, by forcing the cultivator to work to aerate the soil. Illustrations of a curious maze, formed of yew hedges at Hampton Court, pruning and keeping trees from insects, chrysanthemum culture, and practical information on fruits and flowers are among the topics treated. Sample copies may be had of the publishers, Thomas Meehan & Sons, Germantown, Philadelphia.

In the January *Monist*, of importance to students of evolution will be the article on *Germinal Selection*, by the famous German biologist, Prof. August Weismann, of Freiburg. In the theory of germinal selection, Prof. Weismann propounds a doctrine which rounds off and perfects, as he claims, the theories of Darwin and Wallace, and which consists essentially in applying the principle of the struggle for life to the minutest parts of organization, viz., to the germinal and determinant particles generally. Weismann's article is a complete summary of the present status of the discussions in evolutionary theory, and will itself doubtless constitute one of the most important recent acquisitions to biological science.

Abnormal pleasures and pains are treated by Prof. Th. Ribot, who applies to their explanation the pathological method, using diseases as a means of analysis. His results as regards the pleasure which some people take in pain are highly interesting.

The fourth annual meeting of University Extension and other students will be held in the four weeks beginning July 6, 1896, in the buildings of the University of Pennsylvania. The Summer Meeting combines the advantages of an ordinary summer school with the cooperative feature which distinguishes conventions, or associations, in which there are representatives of many universities and colleges.

Professor E. Selenka, of Erlangen, has resigned his position in order that he may make a scientific journey. He has been appointed Honorary Professor of Zoology in Munich. His place at Erlangen is temporarily filled by Dr. Albert Fleischmann.

The Paris Academy of Science has recently elected the following corresponding members: Dr. G. Retzius, of Stockholm, as successor to Carl Vogt; and Prof. R. Bergh, of Copenhagen, as successor to Huxley.

Dr. F. Miescher, Professor of Physiology in the University of Basel, died at Davos, Switzerland, Aug. 26, 1895, aged 51 years. Dr. Rudolf Metzner, of Freiburg i B, has been appointed his successor.

Dr. Felix Hoppe-Seyler, Professor of Physiological Chemistry in the University of Strassburg, died in Wassenburg, on the Lake of Constance, Aug. 11, 1895, aged 70 years.

The Australian Association for the Advancement of Science, will hold its annual meeting at Sydney, Jan. 3 to 10, 1896. Professor A. Liversedge is the President.

The Berlin Academy of Science has elected Professors K. W. von-Gümbel, K. A. von Zittel, A. Cossa, and Mr. Alexander Agassiz as corresponding members.

Dr. R. Krause, who formerly had charge of the Anthropological Section of the Museum Godfroy in Hamburg, died in Schwerin, Mecklinburg, July 25, 1895.

Dr. A. Schaper, of Zürich, has been appointed instructor in Histology and Embryology in the Harvard Medical School.

Ernst Baumann, formerly head of the station of Misahöhe, West Africa, died in Cologne, Sept. 4, 1895, aged 24 years.

Professor Hellriegel, botanist and Director of the Agricultural Experiment Station in Bernburg, died Sept. 24, 1895.

F. Nies, Professor of Mineralogy and Geology in the Agricultural School of Hohenheim, is dead at the age of 56.

Dr. Herman Credner has been advanced to the Ordinary Professorship of Geology in the University of Leipzig.

Dr. V. Rohon has been appointed Extraordinary Professor of Histology in the Bohemian University in Prag.

Dr. Valentin Häcker has been advanced to Extraordinary Professor of Zoology in the University of Freiburg.

Dr. Emil Yung is the successor of the late Carl Vogt as Professor of Zoology in the University of Geneva.

Dr. Moritz Willkomm, formerly Professor of Botany in Prag, died Aug. 26, in Wortenburg, Bohemia.

Dr. Kallies of Göttingen, has been promoted to Extraordinary Professor of Anatomy in Tübingen.

Joseph Thompson, African explorer and geologist, died in London, Aug. 2, 1895, aged 37 years.

Dr. F. Reinitzer, of Prag, has been appointed Extraordinary Professor of Botany in Graz.

Mr. R. Trimen has resigned his position as Director of the Cape Town (Africa) Museum.

L. Perry Arcas, entomologist, died in Requena, Spain, Sept. 24 1895, aged 70 years.

Dr. D. Brandza, Professor of Botany in Bucharest, died August 15, 1895, aged 48 years.

Dr. A. S. Dogiel, of Tomsk, goes to St. Petersburg as Professor of Histology.

Dr. H. Lenk, of Leipzig, has been called to the chair of Geology in Erlangen.

F. Kitton, the student of diatoms, died at Norwich, England, July 22, 1895.

E. J. Chapman, Professor of Geology in Toronto, has resigned his position.

Dr. F. Czapek is now Privat-docent in Botany in the University of Vienna.

Professor Sven Loven, of Stockholm, died Sept. 4, 1895, aged 86 years.

Dr. H. Strahl has been appointed Professor of Anatomy in Giessen.

Dr. P. H. Macgillivray, the student of Australian Polyzoa, is dead.

Dr. M. Miyoshi has been called to the chair of Botany in Tokyo.

Dr. A. Senoner, geologist, died in Vienna, Aug. 30, 1895.

Dr. J. Vesque, botanist, of Vincennes, France, is dead.

Dr. F. Müller, herpetologist, died at Basel in May.

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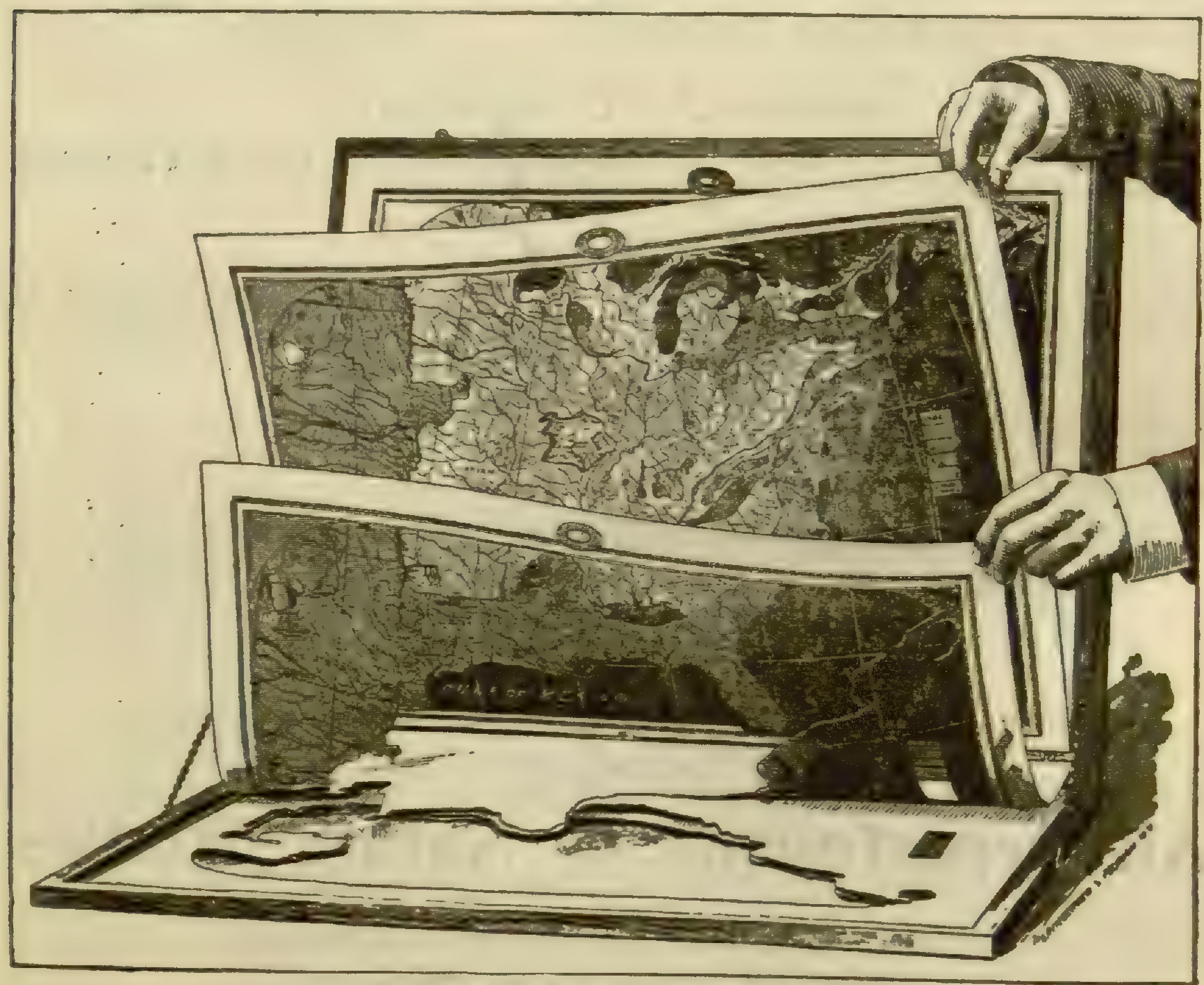

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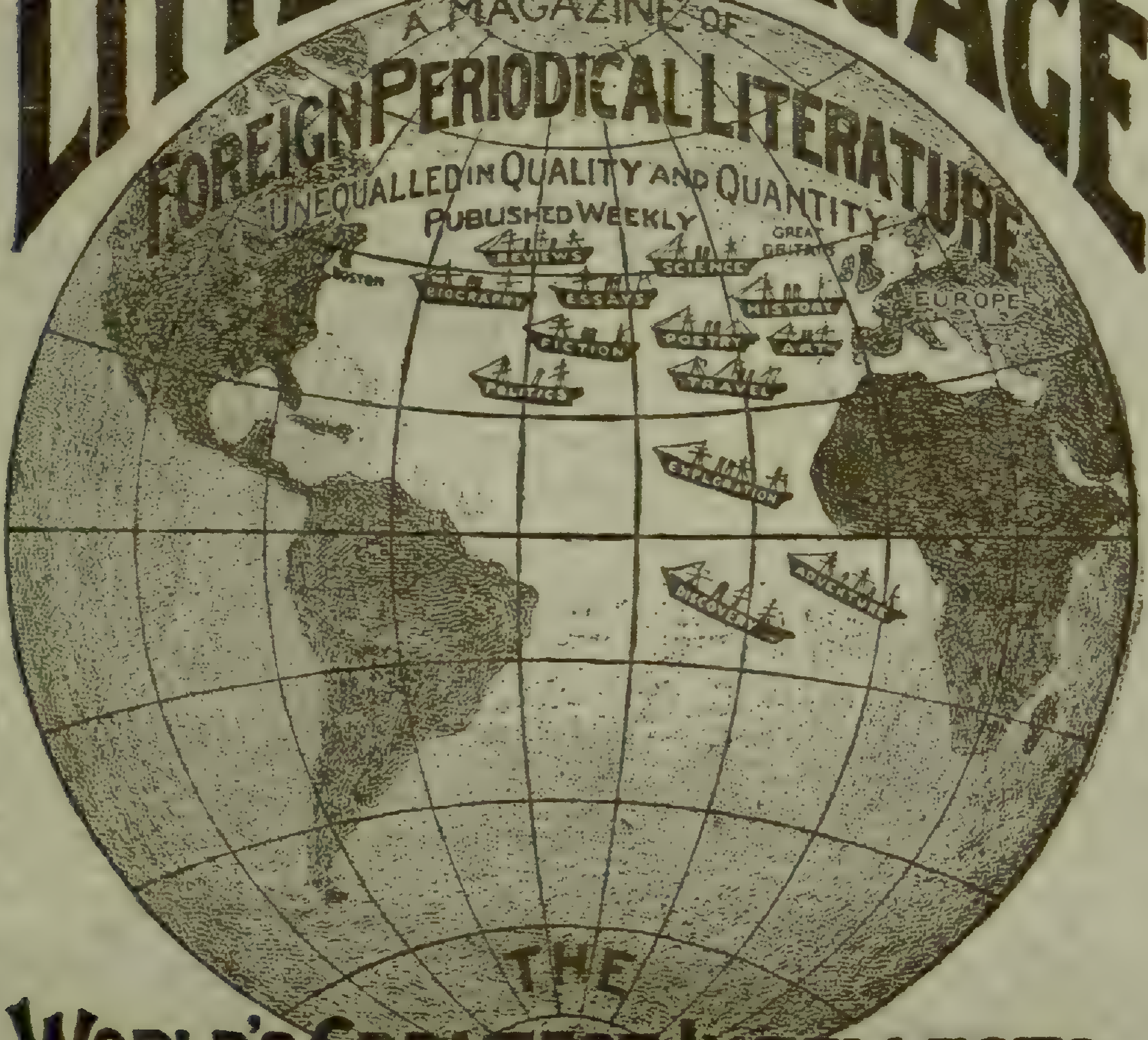

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THE BEARING OF THE ORIGIN AND DIFFERENTIATION OF THE SEX CELLS IN CYMATOGASTER ON THE IDEA OF THE CONTINUITY OF THE GERM PLASM.[1]

Carl H. Eigenmann.

At the meeting of the American Microscopical Society last August I read a paper on the Evolution of Sex in Cymatogaster, of which the present paper is a part. It is not, and was not intended as a full discussion of heredity, but contains observations and conclusions forced upon me while tracing the sex cells from one generation to the next in *Cymatogaster aggregatus* Gibbons, one of the viviparous perches of California.

Since writing it, I have received from Dr. Minot his article "Ueber die Vererbung und Verjüngung," which is just being republished in the Naturalist. I have thought best to present my results as read at the Ithaca meeting, with a note written after the receipt of Dr. Minot's article, although the details of the observations on which the conclusions are based may not appear for some time.

[1] Contributions from the Zoological Laboratory of the Indiana University, No. 12.

The origin of the heredity cells may be explained in one of three ways:[2]

I. The sex cell is the product of the whole organism, and is in this apart from the other tissues. This is the *Pangenesis* of Darwin.

II. The sex cell is an unchanged but increased part of the sex cell of the previous generation, and something apart from the rest of the body. This is *Jaegerism*, or, more popularly, *Weismannism*, and, according to it, the body has no influence over the hereditary cells and changes arising during the life of one individual cannot be transmitted to the next generation.

III. The sex cell is the product of histogenesis and of precisely the same significance and origin as any other cell in the body. This view is held by Morgan, Minot and myself.

As a corollary of the last two is the fact that "in the ancestry of the individual cells of which our body is composed there has never been a death."

The first two theories are not based on observation. They have been evolved from the attempts to explain the heredity power of the sex cells.

The idea of the cellular continuity of successive generations first suggested by Nussbaum in 1880, is now generally accepted. Indeed, there is, perhaps, now no one who would contend that the reproductive cells are new formations in the individual. The reproductive cells are known to be of the same origin as the retinal or any other series of cells. There is but little less unanimity over the idea of the continuity of the unchanged germ plasm, although the number of observations bearing on this point have, necessarily, been very limited.[3] So often is the idea restated without actual examination of the data, the whole subject has become hackneyed. I have taken up this subject because it seems to me the conditions observed in Cymatogaster warrant a conclusion differing from the one generally accepted.

[2] See Osborn, Am. Nat., 1892. Morgan, Animal Life and Intelligence, 1891, p. 131.

[3] Boveri, Befruchtung in Ergebnisse der Anatomie und Entwicklungsgesch, I, 1892, records an apparent case of unchanged transmission.

There is no doubt concerning the continuity of the reproductive cells in Cymatogaster; they may be followed from very early conditions till sexual maturity without once losing their identity. No somatic cells are transformed into reproductive cells, and the comparative constancy of the number of the latter present in any embryo up to 7 mm. long makes it probable that none[4] are ever changed into any other structure. These statements apply with equal force to other tissues.

The difference between the reproductive and the somatic cells is that the latter, after development has begun, continue to develop, divide, grow and adapt themselves to their new duties without intermission. The sex cells, on the other hand, stop dividing at a certain point and remain at apparent rest for a long period. Owing to this arrest in division the sex cells soon stand out prominently as large cells among the smaller somatic cells. Such an arrest in segmentation has been observed in a number of other animals in which the reproductive cells are early segregated, and it cannot be without meaning. It has been supposed that during such periods of apparent rest the cells remain dormant, retaining their embryonic character unchanged. I do not think this is the true reason for the difference of development between the soma and the reproductive cells. The reason seems to me to lie in the fact that the sexual organs are the last to become functional, and their development is consequently retarded. The sex cells, when first segregated—that is, when they first lag behind in segmentation—are not exactly like the ovum from which they have been derived, and there is just as true histogenesis in their development into the reproductive tissues as in the case of any other embryonic cells into their corresponding tissue. Even during the long period of rest from segmentation, the process of tissue differentiation produces a visible and measurable change. But the difference between embryonic cells and undifferentiated reproductive cells being small, the histogenic changes in them during early stages is correspondingly small. This small change has been supposed to amount to no change, and has given rise to that fascinating "myth", the

[4] For possible exceptions see Eigenmann, Journ. Morph., V, No. 3, 1891.

hypothesis of the continuity of unchanged germ cells, and later, when observation in other animals had made this theory untenable, to the theory of the continuity of unchanged germ plasm which is beyond the ken of direct observation.

If the sex cells are the result of histogenesis, it will be necessary to explain their peculiar power. They seem to me to be due to the same processes that have given the retinal cells their peculiar properties.

Assimilation, reproduction and the closely allied hereditary power are the diagnostic characters of protoplasm. These, with numerous other powers, such as contractility, conductivity and irritability, are the properties of every protozoan cell. Even here we find that certain of these functions are more or less restricted to definite parts of the cell. In the higher animals this differentiation has gone so far that definite functions predominate in highly specialized cells to almost the exclusion of the other powers.

With this division of labor and the consequent histogenic differentiation of definite cells in the metazoan corm for purposes of contraction, conduction and irritation, we have also the differentiation for heredity, and it would be surprising if we did not.

In lower forms, where the cells of the body often perform many duties, where the division of labor and histogenesis has not been carried to the extreme, many of these cells also retain the hereditary power to a great extent as shown in the power of budding or regeneration.

There seems to he no necessity to conjure up a substance and processes in the genesis of the reproductive tissues different from those obtaining in the muscular tissues.

During the long ages of the rise of animals those possessing sufficiently differentiated contractile tissue to move the corm to food or from danger have survived, and in precisely the same way those corms containing cells capable of developing into other similar corms have survived. Similar causes have operated in producing each tissue.

The sex cells are proven to influence the formation of the sex ridge. The peritoneal cells rise to form the ridge only

when sex cells are present without regard to whether this position is normal or not.[5] If the sex cells thus influence the surrounding tissue, may we not safely assume a reciprocal influence of the surrounding tissues on the reproductive cells?

Sexuality can first be distinguished not by the difference in the sex cells, but by the character of the peritoneal covering. While this difference in the peritoneal covering may be the expression of an invisible difference existing in the reproductive cells, it is quite possible that sex is determined by the body. In frogs, butterflies, etc. the sex determining power of the soma has been experimentally demonstrated. Later it is well known that the character of the sex cells influences the remotest parts of the organism, although we are not at all familiar with the processes by which this is accomplished.

Changes in the sex cells introduced by the body which do not become apparent until the development of the cells into young, seem, therefore, to be not impossible, although we are entirely unable to tell just how such a change might be accomplished.

Since writing the above, I have received, through the kindness of the author, Dr. Minot's "Ueber die Vererbung und Verjüngung." While the views expressed are not identical with those given in the present chapter, there is considerable agreement. Dr. Minot recognizes that the problem of the origin of the reproductive cells is also the problem of the origin of the tissue cells (p. 580), and that "a germ plasm in the Weismannian sense does not exist." So far we agree. According to him all parts inherit from the germ and possess, as well as the reproductive cells, the power of multiplying and morphogenesis, but this power cannot manifest itself on the part of the somatic cells because the conditions of the body prevent it. The conditions are the increased amount of protoplasm and the specialization of the tissues. According to my views it is not so much a high state of tissue differentiation which *holds captive* the morphogenic power in muscle cells for instance as it

[5] In one interesting larva a few of the sex cells were belated in their migration and situated in front of the normal position. Sex ridges (germinal bands) formed about these sex cells entirely independent of and separated from the sex ridges occurring in the normal place.

is the process of tissue differentiation which *emphasizes* the contractile power in the muscle cell, at the same time *limiting* and finally *eliminating* the morphogenic power, and which gives the sex cells morphogenic power in such marked degree while it deprives them largely of contractile power. In a former paper,[6] I stated this view thus: "The segmentation nucleus of metazoa contains, as in the infusoria, both micro and macro nuclear elements, but these are retained in varying proportions in its descendants, *i. e.*, in the cells of the adult organism. Through a process of division of labor the power of rejuvenescence becomes restricted to comparatively few of the cells derived from the segmentation nucleus."

While Minot's views are in part borne out by the conditions in Cymatogaster, the italicised part of the quotation below finds no support, and is negatived by all the observations made in Cymatogaster. His conclusion, as translated by me, is: "Somatic cells are simply cells in which the activity of heredity is prevented by senescence, viz.: tissue differentiation, *but the somatic cells can, under favorable conditions, be translated into the rejuvenated stage and then develop the most complete or, at least, more complete, hereditary power.*"

ABSTRACT OF OBSERVATIONS ON WHICH THE ABOVE CONCLUSIONS ARE BASED.

The sex cells originally segregated retain their individuality, but undergo a measureable change between the time of their segregation and 7 mm. long larvæ. Soon after the larva has reached a length of 7 mm., the sex cells begin to divide. In the meanwhile they have migrated laterad and lie, for the most part, in a longitudinal groove formed by a duplication of the peritoneum into which a few peritoneal cells have also migrated. *In one such case an extra sex ridge was formed much further forward than usual, in connection with a few sex cells which were accidentally belated in their migration.* The peritoneal cells which have migrated into the sex ridge give rise to the entire stroma of the future sex glands, and together with the sex cells form a core quite distinct from the covering

[6] Bull. U. S. Fish Comm., XII, 442, 1894.

of peritoneum. Posteriorly the sex ridges of the two sides are united into a single ridge. There is considerable variation in the rate of segmentation in larvæ of the same size, but the following table will give an idea of the segmentation and the number of cells in successive stages:

Size of larva.	No. of sex cells.	No. of generations from fertilization.
45–5 mm.	9–15	5
8	22	6
10	28–183	6–9
12	39–143	7–9
15–17	638–2280	11–13 sexes distinct.
16–25	2200–8000	13–15

The sexes can first be distinguished not by the differences in the sex cells, but in the tunic of peritoneal cells. A small groove on the outer ventral part of the sex ridge is the first indication of the ovarian cavity and the surest criterion of the female. In the male the sex gland remains much more circular in cross section and no groove is developed. Much later histological differences in the sex cells themselves can be made out. The long slender chromatin threads of the female cell just before dividing are represented in the male by short, thick bars.

THE HISTORY AND PRINCIPLES OF GEOLOGY, AND ITS AIM.

By J. C. Hartzell, Jr., M. S.

(*Continued from page 183.*)

Lamarck and Defrance earnestly engaged in study of fossil shells, and the former, in 1802, reconstructed the system of conchology and introduced into it the new species collected by the latter from the strata underlying the city of Paris and quarried for the construction of its buildings. Six years previous to this Cuvier had established the different specific character of fossil and living elephants and he devoted himself to researches throughout the remainder of his life. Jameson, in 1808, pointed out the nature of all the rocks and the mode in which they were formed, and made use of the observations

of Desmorest, who, in 1768, traced the origin of basalt to the crater of volcanoes.

In 1807 the Geological Society of London was established with the professed object of encouraging the collection of data and the making of observations. In 1819 the Society published a map of England by the aid of Greenough. About the same time Buch prepared a similar map of a large part of Germany. A geological survey of France was ordered in 1822 by the French government, and as a result a geological map was published in 1841. Conybeare and Phillips published a treatise on the "Geology of England and Wales," in 1821. In 1814 Aiken published his work on mineralogy, which had a large circulation at home and in this country. Previous to this Sowerby published a work on "British Mineralogy, illustrated with colored plates," but the date of which I do not know. The publication of the Geological Map of England, in 1815, by Smith, may be said to form an epoch in the history of geology.

In 1809 Maclure published an article on "Observations on the Geology of the U. S., explanatory of a Geological Map," and he is rightly called the father of American geology. He visited all parts of the Union and all the principleming ni districts of Europe. In 1817 he presented a report to the "Philosophical Society of Philadelphia" of his work, and accompanied it with a colored map. In 1816 and 1817 he visited the Antilles and published a paper on their geology. In 1810 Bruce, of New York, published the first purely scientific journal supported by original American contributions. His journal was devoted principally to mineralogy and geology. Science was also promoted by the collections in the colleges and societies, and by those made by scientific men. In 1816 Cleveland published a treatise on mineralogy. In 1818 Dana published a detailed report on the mineralogy and geology of Boston and vicinity. In the same year the American Journal of Science was first published. The first geological survey made by State authority was that of North Carolina in 1824.

In 1830 the Principles of Geology, by Lyell, appeared and has most powerfully influenced the direction of scientific

thought in the 19th century. It broke down the belief in the necessity of stupendous convulsions in past times. He adopted and improved the views of Hutton, eliminating the baseless theories mingled with them. He rendered great service in elucidating North American geology, and published his travels on this continent in 1845 and 1849. His "Geological Evidences of the Antiquity of Man," published in 1863, startled the public by its advocacy of Darwin's theory in the "Origin of Species."

And so the science has advanced with rapid strides and is solving the problems that are constantly arising in regard to our planet, and upon its fixed data are based many of the fundamental principles of philosophy.

Having considered the history of the progress of geology, let us now consider its aim and the fundamental principles upon which the geologist bases his work.

In the broadest sense, geology is the science whose province is the planet upon which we live, its history from the beginning to the present, including changes which have occurred in regard to the condition at different periods, its several physiographic features, its atmosphere, temperatures, and aqueous bodies, and its life at different stages. In a nutshell, the evolutionary progress of the earth.

The narrow or commonly accepted view does not consider the changes that have occurred, other than those that occurred to the visible portion of the earth. Back of what is supposed to be the earliest formation, it does not attempt to go.

The latter view is sufficient for the ordinary geologist or for the geologist who does not care to speculate on hypotheses which refer to the origin of the earth; but to the geologist who is anxious to grapple with problems which require a drawing upon the imagination for solution, this is not enough. Chemists are not satisfied to study a drop of water, but they are anxious to know its origin; its composition is not sufficient for them. Botanists and zoologists desire to know the origin of plants and animals, not merely their structural and physiological features.

Geologists who study the earth, not merely to satisfy their own curiosity as to the *present* condition of things, but for the purpose of advancing the science, and unraveling the mysteries of the past, in order to produce a history of the planet as accurately as human knowledge in its present condition will permit, are only satisfied with the broad and comprehensive view.

Geology, by the aid of astronomy and physics, therefore, begins with a great nebulous mass, of which all celestial bodies were component parts. It traces the evolution of each body, and that of the earth in particular. Starting when the earth was thrown off as a ring of cloudy or gaseous elements, it traces it through its transformation into a sphere of molten matter surrounded with gases, through which the parent body, the sun, could not penetrate. We learn of the war that existed between the congealing surface and the liquid interior in which the former came off victorious, and formed a crust through which the latter seldom broke. Then began the war between the condensing vapors and the heated crust, in which the latter succumbed to the overpowering element that fell upon it and fairly covered it.

Geology tells us of the life that existed in this mighty ocean after it became sufficiently cooled, and in the powerful internal movements that resulted in the upheaval of masses of rock that were to be the nuclei of the present continents, the history and the formation of which is traced with great minuteness, and the life of each is described with great care, from the lowest forms to the highest, and also the period in which each form lived.

There are several principles by which the geologist is guided in answering the questions that continually arise as he studies the earth with its many characteristics.

1 In the first place, he understands that *geology is an inductive science.* That is, it is a process of demonstration in which a general truth is gathered from an examination of a self-evident truth. Let me illustrate: From the study of modern glaciers he learns certain facts in regard to conditions necessary for their formation, their modes of action, and the results of those actions.

Now, whenever a geologist sees the results of some great force and those results are similar to the phenomena produced by glaciers, he concludes that at some previous time the conditions were such as to make it possible for glaciers to exist in the locality in which his observations were made, for no other force could produce them.

2 He reasons that *all affects must be referred to secondary causes.* In other words, law governs all phenomena, and forces are so balanced as to produce all *known* and *unknown phenomena.* All events that have transpired in the development and configuration of the earth have been brought about by law. In the formation of glaciers certain laws are obeyed, and those laws are always obeyed unless an equilibrium is sustained between them and some other laws are overbalanced.

When the conditions are favorable for the action of *glacial laws* glaciers will be found. The same principle holds good in the distribution of life.

3. *The forces in existence to-day are capable of producing all phenomena that have and may occur.* Therefore, the geologist must study the methods by which they are producing changes at present, and thereby be able to judge of what took place ages ago, and the manner in which great events transpired. In other words, *the past is understood by the present* and to some extent *the future may also be understood.* No new law is, nor has been, necessary for the explanation of phenomena and, therefore, there have been no accidental happenings. There may be laws that man has not as yet learned the nature of, and they may be so balanced as to be beyond man's comprehension, but that there are being or have been created new laws, and that there are accidents, the geologist does not admit.

4. *The earth is undergoing and therefore has undergone changes.* He sees this in studying the phenomena of denudation and disintegration. He sees that the mountains are being destroyed by chemical and physical agencies, and that they are being gradually carried into the valleys, and then into the sea. This, he reasons, must have been going on ever since the first continent made its apppearance.

5. Finally, from a consideration of the above principles, the geologist realizes that his *work must be systematic*, and that the bulk of it *must be done in the field. Field investigation is indispensable. Laboratary work holds a subordinate position.*

It is safe to say that geology has advanced more rapidly than any other science, and the number of those who are making a specialty is steadily growing. New periodicals devoted to the science are continually appearing, and its literature is quite comprehensive. Very little attention was paid to it in our colleges at no late date, but to-day it occupies a prominent position.

The great advance which has been made is due to systematic field work, followed by laboratory work, and the latter is of but little value from a geological standpoint unless it is based upon accurate field investigation. It is necessary to reduce to a practical formula the data secured in the field, and to have a definite method of procedure, for without such, much time is wasted, and many results that otherwise would have been valuable are entirely lost. Mere conjecture must not be indulged in, but "work persistently back from the seen and known to the unseen and unknown," should be the maxim. Conclusions must not be arrived at too hastily. Professor Dana once said, "I think it better to doubt until you know. Too many people assert, and then let others doubt." Hence, in drawing conclusions from the results of field and laboratory work, be sure you are right, before giving publicity to them, and if a doubt exists, state it, and be willing to change your theory. Dana says, "I always like to change when I can make a change for the better."

It is obvious, from what I have said, that geology is a field science. Different characteristics of the earth's surface cannot always be taken into the laboratory for study at leisure, and it is necessary to see the objects under study if we would arrive at correct conclusions and fix them indelibly in our minds. Facts then become real, and we acquire a correct understanding in regard to the forces that have been at work preparing this planet for man.

It is necessary to have a knowledge of other sciences if one would make practical use of geology, that is, to understand the many phenomena that are presented to him.

Natural philosophy and chemistry are necessary in order to determine the composition of rocks and to understand how they were formed and changed. Botany is necessary to understand paleobotany, zoölogy is necessary to understand paleozoölogy, astronomy figures very prominently in the determination of the relations of this planet to other heavenly bodies. Anything that the telescope and the spectroscope reveal is of geological importance, and bears upon the past and future condition of the earth. Mathematics is constantly in use, and without that science little or nothing could be accomplished.

The foundation work of a geologist, therefore, should be a knowledge of the natural sciences, for without them he will be materially hampered in his work.

Geology is practical as well as literary in nature. Every agriculturalist would become more scientific, and would reap better "crops" if he had a knowledge of the science, for it gives a knowledge of soils and fertilizers. To the engineer it is of great importance, for thereby he understands drainage and the best methods for excavating. It is of great importance to the manufacturer, for he can better understand clays, ores, fuels, etc., and in mining it is of great value for it enables the miner to understand the nature of the rock in which the metals occur and assists him in "prospecting."

This use of the science is termed "Economic Geology" and is of inestimable value and importance in developing systematically the resources of a state or of a nation.

The United States government has realized the importance of thorough and accurate investigation of this vast country of ours from an economic standpoint, and established the U. S. Geo. Survey in 1879 for this purpose. Most of the states have their surveys and work for the same ends, but on a smaller scale, and assist, and are assisted by, the government survey, and so work in harmony with each other.

Individuals are at work gathering information in regard to particular formations, correcting mistakes, advancing new theories, devising new plans for more thorough and accurate works and imbuing students with the grandeur of the science.

What is there more sublime than a science that reveals the universe in all its beauty and grandeur and as the result of the balancing of forces which emanate from a creative will? Geology reviews the history of the planet from the earliest known formation to the present. Back of this it goes by retrograde calculation, and hence we have a complete resumé from the time "the earth was without form and void," to the phenomena observed to-day. It tells us of periods of time of immeasurable duration, during which was being molded that upon which it would be possible for life to exist, and over which mind should rule.

There is no science which presents so many problems to be studied, nor in which so much of interest can be taken. It carries one over plains, up the rugged mountains and down into valleys. On every hand is found something new upon which to concentrate the mind, and which demands a satisfactory explanation. How came these plains, these mountains, these valleys? How came those masses of rock, thousands of feet high? Why is sandstone here, limestone there, and granite yonder? What mean those remains of animals and plants that are not in existence to-day? Why are those masses of rock in every conceivable position? Whence came the waters and the land? The plants and animals? Is there a reason for all we see? Are these things accidental, or was there a purpose in their formation?

And so questions crowd upon us, and fill us with wonder and admiration, and with a determination not to be satisfied until they are answered. We see that law is at work, fashioning the universe, and we have brought very forcibly to our minds the fact that there was a purpose involved in the creation of the universe, and that from this realized grand conception is being evolved a divine purpose. That which at first appeared to be outside the domain of law, is seen to be the result of the balancing of forces; and we come to realize the fact

that law pervades the universe, and although we do not know as yet the way in which these laws are balanced to produce all phenomena, that they are so balanced as to produce harmony, and that in proportion as the human mind develops it will be capable of grappling with problems that are not now within its reach.

LIFE BEFORE FOSSILS.

By Charles Morris.

(*Continued from page* 188.)

Such a new stage of existence may have been essayed frequently. The dwellers in the early seas, in their descents below the surface, must often have come into contact with the bottom, and at times temporarily rested upon it. This contact with hard substance doubtless produced some effect upon them, and certain variations in structure may have proved of advantage in these new circumstances and been retained and further developed. Particularly if food was found there, and habitation on or near the bottom was thus encouraged, would such favoring variations tend to be preserved.

But, as has been said, myriads of years may have passed in the slow development of swimming pelagic animals before this phase of evolution was completed. And, perhaps, not until this was fully accomplished did contact with the bottom set in train a new series of changes, and in time give rise to the greatly transformed bottom-dwellers. The change, indeed, was a great one, if we may judge by the wide diversity in character between the swimming embryos and the mature forms of oceanic invertebrates, and must have needed a long period of contact with the bottom for its completion. Yet it was probably much more rapid than had been the preceding pelagic development. Contact with solid substance was a decided change in condition, and may have greatly increased

the preservation of favorable variations. And the area of habitation on the single plane of the sea bottom is so restricted as compared with that within the many planes of oceanic waters, that the struggle for place and food must have been greatly increased, and the development and preservation of newly adapted forms have been more rapid in consequence.

This may seem to bring us to the very verge of the kingdom of life as it is known to us from the oldest fossils yet discovered. Yet in truth we are probably still remote from it. We are still dealing with soft bodied animals, not with those possessed of the hard external skeletons from which fossils are produced. There is no good reason to believe that mere contact with the earth induced the previously naked swimmers to clothe themselves in solid shells. In truth, the earliest bottom-dwellers may have long continued soft bodied, the hard case or shell being only slowly evolved. The mantle of the mollusk, for instance, with its shell-secreting glands, is not likely to have been a primary accessory of molluscan organization. The same may be said of the chitin-forming glands of the crustacea, and the analogous glandular organs of other types. Such conditions must have developed slowly, and their appearance was probably due to an exigency of equally slow unfoldment.

For now we come to another highly important problem, that of the true disposing cause of the development of dermal skeletons, on which there exists some basis for speculation. In truth the fossils preserved for us in the Cambrian rocks have an interesting tale to tell which has a strong bearing upon the story of animal evolution. And this is, that all these bottom-dwellers, with the exception of the burrowing annelids, became covered with what was probably defensive armor. They all seem to have sought protection in one way or other, and in so doing became in a measure degenerated forms of life, their former ease of motion being now partly or wholly lost.

All this represents an interesting stage in the process of evolution, and indicates some special exigency in life conditions which the animals of that age could only meet by rendering themselves heavy and sluggish with a weight of inclosing

armor. This new phase of evolution may have proceeded very rapidly, many forms of early life disappearing, while those that quickly became armored survived.

What was this exigency? Protection, apparently, as is above stated. But protection from what? Against what destructive foe did these ancient animals need such strong defence? Which among them was the rapacious creature whose ravages imperilled the existence of all the others? Certainly not the sponge or the cœlenterate; they feed on smaller prey. The mollusk or the echinoderm, in their agile unclad state, may have been actively predatory, but they were among those forced to seek protection. Of the known forms the trilobite seems most likely to have been the aggressive foe in question. It was the largest, the most abundant, and, perhaps, the most active of them all, its size and numbers indicating an abundance of easily obtained food, while its great variety of species points to the existence of varied conditions of food or methods in food getting.

To all appearances the trilobite was then the lord of life, the Napoleon of that early empire. Awkward and clumsy as such a creature would appear now, it was then superior in size, strength, and probable agility to all other known animals, while its numbers and variety indicate that it was widely distributed and exposed to all the varying conditions of existence at that time. What a hurrying and scurrying there must have been among those small soft creatures to escape this terrible enemy, from whose assaults nothing seems to have availed them but an indurated external covering, too hard for its soft jaws to master. As the prey became protected in this manner the destroyer probably improved in strength of jaw, and there may have been a successively more complete growth of protective devices in the prey and of powers of mastication in the foe. And thus arose the conditions which first made fossilization possible, in the development of a series of armor-clad creatures which were really late comers upon the stage of life, remote as they seem when measured by our standard of time.

But the story is only half told. The trilobite, as it is known to us, is under armor also. Not only is it clothed in a dermal

skeleton, but, in its later forms, is capable of rolling up into a hard ball with no part of its body exposed. Evidently the destroyer himself in time came into peril and needed protection. Some still more powerful and voracious foe had come upon the field, and the triumphant trilobite was forced to acknowledge defeat.

We cannot well imagine any of these animals assuming such armor except for protective purposes. The weight laid upon them rendered them slow and sluggish, fixed some of them immovably, and greatly decreased their powers of foraging. The only cause which seems sufficient for their assuming this disadvantageous condition is that of imminent peril—a peril which affected all known forms alike.

Whence came this peril? Where is the voracious foe against whom they all put on armor, even the preceding master of the seas? No trace of such a creature has been found. In truth, we cannot fairly expect to find it, since it was probably destitute of hard parts, and left behind it nothing to be fossilized. It had no foe and needed no armor, while lightness and flexibility may have been of such advantage to it that armor would have proved a hindrance. It probably was a swimming creature and thus left no impress of its form upon the mud. It is to this unknown creature that we must ascribe the armored condition of all known forms of life at that period, even the later cephalopods, large and powerful mollusks, becoming clothed in a cumbrous defensive shell, which they were obliged to drag about with them wherever they went.

It is a strange state of affairs which thus unfolds before our eyes. All the life we know of seems diligently arming itself against some terrible enemy, which itself has utterly vanished and left as the only evidence of its existence this display of universal dread. The creature in question would appear to have been without internal or external hard skeleton and without teeth, trusting to indurated jaws for mastication. At a later date, when its prey became less easily destroyed, teeth may have developed, and it is possible that we have remains of them in the hard, cone-like, minute substances found in the lower Silurian strata, and known as conodonts.

If we may try and rebuild this vanished beast of prey from conjecture, aided by collateral evidence, we should consider it an elongated, flexible form, developed from some swimming worm-like ancestor, perhaps like the Ascidian embryo, stiffened internally by a cord of firm flesh extending lengthwise through the body, and moving not by cilia, but by the aid of fleshy side flaps, the progenitors of the fin. We conjecture it to have been, in short, the early stage of the fish, a creature perhaps of considerable size and strength, due to the abundance of easily obtained food, but as destitute of hard parts and as little likely to be fossilized as Amphioxus.

We may offer this conjecture with some safety, for it is not long before we come upon actual traces of fish, and of a degree of development which indicates a long preceding stage of evolution. In fact, the fish in time appears to have been forced to put on armor, as its prey had earlier done. Internicine war began in the fish tribe itself. A wide specific variation arose, with great differences in size and strength, the stronger attacked the weaker species, and eventually two distinct types of fish appeared, the Elasmobranch and the Ganoid; the former, represented to us by the modern sharks, being much the most powerful and voracious, and holding the empire of the open seas, while the latter dwelt in shallower waters. The Ganoids, preying on the bottom forms, become themselves the prey of their strong and active kindred, and, as a result, the evolutionary process just described was resumed. The weaker fish put on armor, in many cases heavy and cumbrous, a dense bony covering which must have greatly reduced their nimbleness, but which safety imperatively demanded. It is these armored forms that first appear to us as vertebrate fossils; the first fish, as the first mollusk or crinoid known to us, being the resultant of a very long course of development. As regards the Elasmobranchs, they, too, became in a measure protected, though not sufficiently to indicate any very active warfare among themselves.

There is little more which we can say in this connection. The story of the evolution of life bears an analogy worth mentioning to that of the development of arms of offense and de-

fense among men. After thousands of years of war with unarmored bodies, men began to use defensive armor, the body becoming more and more covered, until it was completely clothed in iron mail, and became rigid and sluggish. In the subsequent period offensive weapons became able to pierce this iron covering, and it was finally thrown aside as cumbrous and useless. A similar process is now going on in the case of war vessels, they being clad in heavy armor, which may yet be rendered useless by the development of cannon of superior piercing powers, and be discarded in favor of the light and nimble unarmored ship.

The analogy to animal evolution in this is singularly close. After long ages of active warfare between naked animals, defensive armor was assumed by nearly every type of life, except the lowest, highly prolific forms, and the highest, which had no foes to fear. But the powers of offense grew also, and in time the employment of armor ceased, as no longer available, its last important instance being that of the ganoid fishes. The later fish reduced their armor to thin scales, and gained speed and flexibility in proportion, while in land animals armor was seldom assumed. In several instances creatures have gone back to the old idea, as in the armadillo, the porcupine, the turtle, etc., but the thinly clad, agile form has become the rule, armor no longer yielding the benefit that was derived from it in the days of weak powers of offense. This result is a fortunate one, since with increase of agility mental quickness has come into play, the result being a development of the mind in place of the old development that was almost wholly confined to the body. In the highest form of all, that of man, physical variation has almost ceased, in consequence of the superior activity of mental evolution.

In conclusion it must be admitted that there are certain formations in nature which seem to militate against the argument here advanced. I have already spoken of the much questioned Eozoon canadense. In addition there are the beds of limestone and graphite in the Laurentian formation. But these prove too much for the advocates of their organic origin. If so large a fossil as Eozoon had appeared so early, the subse-

quent barrenness of the rocks would be incomprehensible. And had coral animals and large plants capable of producing such masses of limestone and graphite existed so early, the absence of any fossils earlier than the Cambrian would be inexplicable. It is acknowledged, however, that such formations might have been produced by inorganic agencies, and the facts strongly indicate that such was their origin, and that fossils began to be preserved very shortly after the power in animals to secrete hard skeletons appeared.

BIRDS OF NEW GUINEA (FLY CATCHERS AND OTHERS).

By G. S. Mead.

(*Continued from page* 195.)

The Thickheads (*Pachycephala*) are of many species and scattered widely over the Archipelago. Many have come under trained observation only during recent years. Probably many more await discovery.

Pachycephalopsis poliosoma, Gray Thickhead, was discovered by Mr. A. Goldie in Southeastern New Guinea, and owing to its distinctive coloration was classed as a new genus. It is really one of a group of birds which might form a subgenus and is accordingly so divided by Mr. Gadow. Above the general color is dark gray, almost brown, with the head still darker. The square, rather short tail is also dull of hue. Beneath is dull gray, lighter on the abdomen and tail coverts, whitish to white on the jugulum, throat, chin and side face. It is a pretty, soft colored little bird about 6 inches long, sufficiently numerous among the mountains of the Astrolabe range to be called common.

Pachycephala melanura ranges widely over Northern Australia and the Archipelago. The general color above is olive-green ; wing coverts, tail, head and an irregular band passing

over the head, neck and breast, black and glossy black. The under parts, with a broken collar about the neck, are a warm light yellow. Throat a pure white. Whitish lines the under side of the wings and tail. Bill and feet black. The female lacks the vivid coloring of the male, being brownish where he is a jet black, buff or whitish where he is a bright gold. Length 7 inches.

Very like the above, but of reduced size, is *Pachycephala schlegelii*, whose total length is under 5.5 inches. The differences lie in the greater width of black band across the breast, in the line of black edging the wings, and the orange rufous on the abdomen. The female resembles the female of *Pachycephala soror*, found also among the Arfak Mountains. This bird is olivebrown above, wings and head darker. The under surface is a bright yellow, omitting the grayish wings and dull thighs. Like her mate, the throat and chin are white. The male *P. soror* is unmarked by the yellow nuchal collar but is not without the black crescent. A bright yellow covers the breast and abdomen. The head is black, the tail dusky. Total length about 6 inches.

There are several other species of *Pachycephala* resident in Papua, almost all bearing a greater or less resemblance to each other. Among these may be mentioned without detailed description, *P. hyperythra* from Southeastern New Guinea whose under parts are of the warm reddish color that gives it its specific name.

P. albispecularis, from the Arfak region, is another species—a somewhat larger bird than its kind, gray and dark brown in general coloring with white markings on the wings.

Still another is *P. griseiceps* or *virescens*, with local differences, a bird of the average length, somewhat diversified plumage and a mottled head.

Smaller than the foregoing but with throat and chest crescent more distinctly outlined, is *P. leucogaster*, collected in the Motu country. *P. leucostigma*, from the northeast, is considerably mottled, with much rufous on the under parts, the usual white in this instance somewhat discolored, on the throat, and much streaked on the mantle.

Pachycephala fortis has its habitat in the Astrolabe Mountains, though found probably elsewhere in New Guinea. Its total length is nearly 7 inches, colored almost entirely above dark olive, below ashy gray. The head and mantle are dark gray, the tail dusky, the back and wings greenish olive. On the face are gray shadings. White prevails on the abdomen, passing into yellow. The under wings do not differ from the uniform cloudiness but are, if anything, even duller than the body.

Pachycare flavogrisea, set apart from *Pachycephala*, is colored a bluegray above, somewhat varied on the tail and wings by black or white edgings, while the under parts are a "deep, shining yellow, the yellow on the forehead and the sides of the head and neck being separated from the bluegray of the head by a broad dark stripe." Total length 4.5 inches.

If we look for those attractive little birds—the Titmice—in New Guinea, we shall find very few, if any, specimens. One is mentioned in the books, viz., *Xerophila leucopsis*, an Australian species, abundant in Queensland but not so numerous in Southern Papua. The little bird in question has a length of 4 inches. Its general color is brown, ashy above, whitish and yellowish beneath. Along the tail, neck and head the brown is positive; this is true also of the under wings; elsewhere, however, the colors are pale and indistinct, shading off gradually, as on the sides and breast, into a clouded white.

Several species and subspecies of the genus *Cracticus* range between Australia and New Guinea. These are Lanidine birds of good size, strong of beak, black, white or gray of color.

Cracticus quoyi, a typical representative, is one of these distributed pretty generally over North Australia and Southern Papua. It is almost entirely black and blueblack, the only variation being in the shading and lustre. The length is about 14 inches. Sexes alike.

Cracticus cassicus or *personatus* is more peculiarly insular, being confined chiefly to New Guinea and its islands.

The bird is strikingly conspicuous in its contrasted black and white. The former color covers the head and neck, throat and chest, upper wings and tail, excepting the two

middle feathers which are partially white. There are scattered markings, moreover, of black, intermingled with white on the back and wings. All else is a pure white above and beneath. The female is perhaps not of such glossy plumage and has less white on the back. She is also smaller than her mate by half an inch. Total length 13 inches.

Another species from Southeastern New Guinea, collected by Mr. Stone and others, is called *Cracticus mentalis* or *spaldingii*. This Dr. E. P. Ramsay of the Sydney Museum believes to be identical with *C. crassirostris*, a species separated by Count Salvadori from *C. quoyi*, already described, though by some regarded as one and the same. *C. mentalis* is about 10 inches long. The white is banded so as to divide the black of neck and back. Chin black.

In addition to those not very happily named birds—Eupetes—already mentioned in a previous article, two or three species may be briefly described.

Eupetes incertus is colored above a warm ruddy brown, the tail not quite so bright. White, bordered by dusky covers the throat, side face and abdomen. Over the chest and along the side body the plumage is rufous, the under tail coverts buff. Bill and feet are dark. Total length about 7 inches. The mountains of the northwest are the home of this species, as also of *Eupetes leucostictus* whose breast is flecked with white as its name indicates. This *Eupetes* is boldly colored with its chestnutbrown head and mantle, and its glossed dark green body and black wings spotted white on the coverts. Instead, however, of the usual white throat, the throat is black, although there is much white on either side. Black marks, too, lie on the face near the eye, the chin and upper breast. The lower parts are gray with a bluish tinge. The tail is black, the exterior feathers tipped with white, the middle ones oily green. The bill, feet and eye are black. Altogether this specimen is a remarkably fine one, unlike, in many respects, most of its family.

Eupetes pulcher, discovered in the Astrolabe Mountains, by Mr. Goldie, may be briefly described as differing from *E. castanotus* (Amer. Nat., No. 343, p. 634) only in having the head

a decidedly dusky shade instead of chestnut, and a narrow black edging to the throat in place of a somewhat broad band of black. Length 9 inches. Female a trifle smaller.

Eupetes ajax (Temm.) or *Cinclosoma ajax*, as Dr. Sharpe prefers to call it, classing it as distinct from the *Eupetes*, is a thrushlike bird about the same size as the foregoing. The general color above is a dull brown, becoming darker near and upon the tail and wings. The wing coverts, however, are a shining black; the same is true of the exterior tail feathers, excepting their ends. About the head also there is considerable glossy black which runs down the sides of the neck and becomes the sole color of the throat and upper breast.

White, which appears on the face, is seen on the underparts sometimes rimmed with a streak of black, as on the breast and abdomen, sometimes intermixed with it as on the tail and wing coverts. The sides of the body are of a ruddy tinge.

The general color of *Eupetes nigricrissus* above, including the tail and wings, is bluish, becoming dark, almost black towards the wing extremities, with bluish margins. On the face, especially about the eye there is much black; a band of the same runs around the neck, bordering the pure white throat. White spots the cheeks, also enclosed by black. The under parts are a slate color, with a bluish cast; this is true as well of the tail and under wing. Length 8 inches. The female is similar though a little smaller. The male lacks the clear stripe of white above the eye, which the female possesses. Habitat, Southeastern New Guinea.

Of the *Drymoedus*, a group allied with the *Eupetes*, a species named *Drymoedus beccarii* is the inhabitant of Southern New Guinea and the neighboring islands. The color of this pretty bird is a warm brown above, the head darker, the wings pale brown and black with white tips. The tail is similarly marked. White and black markings diversify the side face about the eye. The rest of the face and throat are clear white. The under parts are a buff, more or less variable; the crissum a dark brown. As on the wings above, so below the coloration contains bars of white in addition to the dusky brown. The bill is black. The length is about 7 inches.

Another bird of kindred species and not very unlike in plumage is *Orthonyx novæguineae.* In this case, however, the white on the under surface is far more extended. This hue is intruded upon by brown and black. The white above is less developed.

Pomatorhinus isidorii of the same family does not differ greatly in appearance. It is rather longer than the preceding and of a prevailing brown or russet, shaded more or less. Its length is about 8 inches. The female is like the male, perhaps a trifle larger in size.

A much smaller genus of birds is *Crateroscelis*, represented in New Guinea by two species, *C. murina* and *C. monarcha.* Here the ground color is still brown, brighter on the tail, darker on the head. Even the throat which is white is slightly tinged. So, too, the abdomen and lower parts generally. Total length 4.5 inches. The latter species has more white upon the under body, otherwise is mainly like the preceding.

RECENT LITERATURE.

Murray's Introduction to the Study of Sea-Weeds.[1]—In this work from the press of Macmillan & Co., George Murray has given us a book which will be of much service to those beginning the study phycology. The introduction treats briefly of the history of phycology, of the geographical and littoral distribution, and the structure of sea-weeds, and there is appended thereto some valuable information on the collection and preservation of material. Following the introduction there is given a well selected list of eighty books and papers on phycology. The book is illustrated by eight full paged colored plates—four on the red, two on the green and two on the brown sea-weeds—and eighty-eight figures in the text. The figures in the colored plates are somewhat crowded, and the specimens figured are in some cases rather

[1] An Introduction to the Study of Sea-weeds, by George Murray, F. R. S. E., F. L. S., Keeper of the department of Botany, British Museum. With eight colored plates and eighty-eight other illustrations. London, Macmillan & Co., and New York, 1895, 271 pp., 12 mo.

fragmentary, but the figures in the text are very good. Most of them having been taken from the recent works of Retuke, Solms-Lauback and the author.

Five sub-classes are recognized, i. e., *Phæophyceæ*, *Chlorophyceæ*, *Diatomaceæ*, *Rhodophyceæ* and *Cyanophyceæ*. The general arrangement of the book is poor; the more complex groups are treated of first and the simpler last, except in the *Rhodophyceæ*, where the reverse order is followed. The *Rhodophyceæ* moreover "present so many difficulties to be understood only after the study of other groups that the author has chosen the *Phæophyceæ* with its familiar forms of sea-wracks and tangles for the first sub-class. The *Chlorophyceæ* and *Diatomaceæ* follow naturally. The *Rhodophyceæ* next make a series by themselves, and finally, come the simple *Cyanophyceæ*. In the *Phæophyceæ* seventeen orders are recognized which are the same as those of Kiellman in Engler and Prantl's *Pflanzenfamilien* with a few exceptions. *Spermatochnus* is placed in the *Sporochnaceæ* and *Myriotrichia* in the *Elachistaceæ* instead of each standing in an order by itself; the *Dictyoteæ* are placed between the *Cutlereaceæ* and *Tilopteridaceæ* instead of being left out altogether; the *Ralfsiaceæ* are placed near the *Sphacelariaceæ* instead of near the *Laminariaceæ* as they have been by Kiellman and others. *Splachnidium*, a monotypic genus found only in the southern oceans, which has until recently been included among the *Fucaceæ*, is placed in an order by itself—the *Splachnidiaceæ*. It has been found that the conceptacles of *Splachnidium* contain sporangia similar to those of the *Laminariaceæ* instead of oospores and antheridia, hence it is placed near that order. The marine *Chlorophyceæ* are treated under eleven orders; many recent facts as to their reproduction being incorporated. At the end of two groups, the *Perecllineæ* and the Coccospheres and Rhabdospheres are briefly mentioned as being on the borderland between the vegetable and animal kingdom. In the twenty pages devoted to the *Diatomaceæ*, the structure, reproduction, geographical and geological distribution are quite fully discussed, but nothing is said of the arrangement of the groups and very little of its systematic position. We can agree with the author that the diatoms should not be placed in the *Phaeophyceæ* solely because they have a coloring matter closely related to that of the brown sea-weeds, but we can hardly agree that a siliceous covering and the presence of diatomine are sufficient to separate so widely two groups otherwise so closely related as the diatoms and desmids.

According to the preface "the account of the *Rhodophyceæ* is based on the scattered papers of Schmitz, who by utilizing his own researches

and the splendid investigations of Thuret and Bornet, has almost wholly altered the classification of the sub-class." Four orders are recognized, based upon the development of the cystocarp; the *Menalionaceæ*, *Gagartinaceæ*, *Rhodomenaceæ*, *Cryptonemiaceæ*. The *Bungiaceæ*, including *Perphyra*, are placed at the end of the *Rhodophyceæ* as an *Anhang*. In the last ten pages the *Cyanophyceæ* are briefly treated under two orders, the *Nostocaceæ* and *Clerocaccaceæ*. Throughout the work each order and in the larger orders each family is synoptically treated under four heads; general character, thallus, reproduction and geographical distribution. In it are embodied the results of the latest investigation on all groups, much having been taken from the able investigations of the author and his associates. Errors are comparatively few, one of the most noticeable being the mentioning of genus *Egregia* as one of the Fucaceae (P. 55). It is again mentioned in its proper place among the *Laminariaceae* (P. 85).

De Alton Saunders.

Taxonomy of the Crinoids.—The true position of a science in the scale of progress is measured by the degree of perfection exhibited in the systematic arrangement of the phenomena of which it treats. Its claims to philosophic recognition are proportional to the accuracy of the genetic relationships shown in its system of classification. If this be true of a general science, it is no less a reality in its various departments. There is, perhaps, nowhere a better exemplification than the Crinoids; and no zoological group has made in recent years more rapid progress towards a rational classification.

The data upon which the systematic arrangement of the stemmed echinoderms rests are elaborately set forth in the lately issued work of Messrs. Charles Wachsmuth and Frank Springer.[2] It is of great interest to know that the advancement in an understanding of the group has been almost wholly from the paleontological side and that the results are accepted practically without change by the most eminent students of the living forms. As is well known, the crinoids are to-day almost extinct; but that in past geological ages they were the most prolific forms of life. On account of the peculiar construction, unusually great opportunities are afforded for the solution of morphological problems, and full advantage has been taken. Upon so firm a foundation does the classification of the crinoids, as prepared by Wachsmuth and Springer now rest, that it is hardly probable that it will require radical change for a century to come.

[2] North American Fossil Crinoidea Camerata: Memoirs Museum Comp. Zool., 2 parts, 800 pp., and atlas of 83 plates. Cambridge, 1895.

As regards the major subdivisions of the stemmed echinoderms three groups are recognized: the cystids, the blastoids, and the crinoids. These are considered as groups of equal rank. The forms of the first are earliest in time, lowest in taxonomic position, and are regarded as the ancestral types of the other two. The crinoid type itself is a very old one, dating from the Cambrian in which it is even then in a high stage of development. During the Ordivician the cystidian features had almost wholly disappeared. The crinoidal group is remarkable for the persistence it has shown in preserving its pentamerous symmetry; and although the introduction of the anal plate so disturbed it as to well nigh produce a permanent bilateral arrangement, the former was finally permanently retained.

Neocrinoidea and Palæocrinoidea, the two primary groups of crinoids which were formerly almost universally recognized, are abandoned. In their stead are recognized three principal subdivisions: Inadunata, Camerata and Articulata. It is quite remarkable that this ternate grouping of the crinoids is essentially the same as Wachsmuth originally proposed more than twenty years ago, and that often being compelled by students of the recent forms to abandon it and to substitute others, a careful survey in the light of recent discoveries of all crinoids both fossil and living has clearly shown that the main subdivisions first suggested are essentially valid and are applicable to all known forms. The criteria for separating the crinoids into orders are briefly as follows:

1. Condition of arms, whether free above the radials or partly incorporated in the calyx.
2. Mode of union between plates of the calyx, whether movable or rigid.
3. Growth of stem, whether new plates are formed beneath the proximal ring of the calyx or beneath the top stem joint.

The simplest forms, the Crinoidea Inadunata, have the dorsal cup composed invariably of only two circlets of plates or three where infrabasals are present; there are no supplementary ossicles except an anal piece, which is, however, not always present; the arms are free from the radials up. In the construction of the ventral disk two different plans are recognizable, and upon these are established two sub-groups, the Larviformia and Fistulata. The former has the disk in its simplest possible form, being composed of five large orals arranged in a pyramid; the second has the ventral side extended into a sac or closed tube often reaching beyond the ends of the arms.

The Camerata are distinguished by the large number of supplementary pieces which bring the proximal arm plates into the calyx, thus enlarging the visceral cavity; all plates are heavy and immovable; the mouth and food grooves are tightly closed.

The Articulata have to some extent the incorporation of the lower arm plates with the calyx, but the plates are movable instead of rigid. The mouth and food grooves are open. The infrabasals are fused with the top stem joint which is not the youngest plate of the stalk. According to whether or not pinnules are present two suborders are recognized: the Pinnata and Impinnata.

An analytical synopsis of the families of Camerata as proposed by the authors and as now understood is as follows:

I. Lower brachials and interbrachials forming an important part of the dorsal cup.

A. Interradials poorly defined.

The lower plates of the rays more or less completely separated from the primary interradials by irregular supplementary pieces; dicyclic or monocyclic — Reteocrinidæ.

B. Interradials well defined.

1. Dicyclic.

a. Radials in contact except at the posterior side — Thysanocrinidæ.

b. Radials separated all around — Rhodocrinidæ.

2. Monocyclic.

a. Radials in contact all around.

Symmetry of the dorsal cup, if not strictly pentamerous, disturbed by the introduction of anals between the brachials only — Melocrinidæ.

Arms borne in compartments formed by partitions attached to tegmen; dorsal cup perfectly pentamerous; plates of calyx limited to a definite number — Calyptocrinidae.

b. Radials in contact except at the posterior side, where they are separated by an anal plate.

First anal plate heptagonal, followed by a second between two interbrachials — Batocrinidae.

First anal plate hexagonal, followed by two interbrachials without a second anal, arms branching from two main trunks by alternate bifurcation — Actinocrinidae.

II. Brachials and interbrachials slightly represented in the dorsal cup.

1. Dicyclic,

Radials in contact except at the posterior side — Crotalocrinidae.

2. Monocyclic.

a. Radials in contact all around; base pentagonal — Platycrinidae.

b. Radials separated at posterior side by an anal plate; base hexagonal.

Basals directly followed by the radials — Hexacrinidae.

Basals separated from radials by accessory pieces — Acrocrinidae.

Regarding the terminology employed, special attention should be called to the clear and concise definitions given of the various structural parts. The terms should be universally adopted, and they form

by far the best collection ever proposed. American writers especially will need no appeal to at once use them, not only in order to secure uniformity in nomenclature but to insure precision of description. Heretofore the names of the various plates or groups of ossicles have been used in a rather haphazard way. Not only have different designations been given to the same part but the same title has been repeatedly applied to structures widely separated morphologically.

CHALES R. KEYES.

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General Notes.

PETROGRAPHY.[1]

Examples of Rock Differentiation.—Yogo Peak in the Little Belt Mountains, Montana, consists of a stock of massivei gneous rock which breaks up through surrounding horizontal sediments, that have been metamorphosed on their contact with the eruptive. A vertical section through the south face of the mountain caused by a branch of Yogo Creek has afforded Weed and Pirsson[2] and excellent opportunity to study the relations of different phases of the eruptive to one another. The massive rock shows a constant variation and gradation in chemical and mineralogical composition along its east and west axis which is two miles in length. In its eastern portion the rock is a syenite, containing pyroxene, hornblende, biotite, orthoclase, oligoclase, quartz and a few accessories. The pyroxene is a pale green diopside and the hornblende a brownish-green variety. The latter is thought to be paramorphic after the former. In structure the syenite is hypidiomorphic with a

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Amer. Journ. Sci., Vol. L, 1895, p. 467.

tendency toward the allotriomorphic structure. Further west, about in the center of the mass, the syenite changes to a darker gray rock with a tinge of green, somewhat resembling a diorite. It is more coarsely crystalline than is the syenite and is much more basic. The minerals are the same as in the syenite, except that quartz is lacking, but differ somewhat in their character and in the proportions present in the two rocks. The augite is now a bright green idiomorphic mineral. Hornblende is rare and biotite abundant. The great difference between this rock, which the authors call yogoite, and the syenite, is in the relative proportions of augite and orthoclase present in them. In the yogoite the pyroxene predominates over the orthoclase, while in the syenite the reverse ratio exists. In the western portion of the rock mass, the prevailing type is shonkinite, a very dark basic rock, very similar to that of Square Butte.[3] Augite and biotite are very abundant as compared with the orthoclase, which in turn predominates over plagioclase. This latter mineral is represented by andesine, a more basic feldspar than that in either the syenite or the yogoite. Analyses of the three types of Yogo Peak rocks follow:

	SiO_2	TiO_2	Al_2O_3	Cr_2O_3	Fe_2O_3	FeO	MnO	MgO	CaO	BaO	SrO	Na_2O	K_2O	H_2O	P_2O_5		Total
Syenite	61.65	.56	15.07	tr	2.03	2.25	.09	3.67	4.61	.27	.10	4.35	4.50	.67	.33	=	100.15
Yogoite	54.42	.80	14.28	tr	3.32	4.13	.10	6.12	7.72	.32	.13	3.44	4.22	.60	.59	=	100.19
Shonkinite	48.98	1.44	12.29	tr	2.88	5.77	.08	9.19	9.65	.43	.08	2.22	4.96	.82	.98	=	99.77

Shonkinite contains in addition .22 per cent. of Fl.

From a consideration of the nature of the three types of rock the authors conclude that the Yogo Peak stock exhibits the results of a progressive differentiation along its major axis. There is a progressive increase in the ferro-magnesian constituents from the east to the west and a consequent increase in basicity. All the components of the three types exhibit the effects of this differentiation in the proportions present in the different rocks. The Yogo Peak mass is thus an illustration of a "Facies suit" as distinguished from a "rock series." In the former differentiation took place in situ, whereas in a 'rock series' differentiation occurred before the eruption of rocks into their existing positions. The facies suit of Yogo Peak together with the rocks of neighboring mountains comprise a distinct rock series.

The authors close their paper with an appeal for a more specific nomenclature in petrography—a nomenclature that will take account not only of the qualitative relations between the minerals that make up rock masses but of the quantitative relations as well. The Yogo Peak

[3] Compare American Naturalist, 1895, p. 737.

rocks form a natural series with sanidinite and peridotites. Rocks composed of orthoclase and no augite = sanidinite; when orthoclase exceeds augite = augite-syenite; when orthoclase equals augite = yogoite; when augite exceeds orthoclase = shonkinite; when augite alone is present = pyroxenite and peridotite. In this scheme the term augite includes also other ferro-magnesian minerals, and the terms orthoclase other feldspars.

In connection with the article above referred to Iddings[4] mentions the existence of a series of rocks associated with typical basalts and andesites in the Yellowstone National Park. They represent like phases of differentiation belonging to separate, but similar rock families. Most all of these rocks are basaltic looking. They occur in flows and dykes and sometimes as breccias, constituting the major portion of the Absaroka Range. These rocks present a wide range of composition within definite limits, forming a series connected by gradual transitions. Three classes are distinguished, the first of which is characterized usually by abundant phenocrysts of olivine and augite and an absence of feldspar phenocrysts; the second class is characterized by the presence of labradorite phenocrysts in addition to those of olivine and augite, and the third class by the presence of labradorite phenocrysts. The names given to the three classes are absarokite, shoshonite and banakite. The distinctions between the classes is based principally upon their chemical relationships. A large number of analyses, most of which were taken from other papers, illustrate their points of difference. A comparison of the analyses, besides showing the close relationships existing between the rocks of the three classes, shows also what mineralogical differences may obtain for rocks of the same chemical composition. The shoshonite from the base of Bison Peak and the banakite from Ishawooa Canyon have practically the same chemical composition. The former, however, contains abundant phenocrysts of labradorite, augite and olivine, while the latter contains numerous labradorite phenocrysts, but few and small ones of the other two minerals. The groundmass of the first shows much less orthoclase than that of the second, and no biotite, which abounds in the second. The author compares the series of rocks studied by him with the series studied by Merrill[5], with the series discussed by Weed and Pirsson and with Brögger's[6] grorudite-tinguaite series. The conclusion reached by this comparison is to the effect that it may be doubted whether the gen-

[4] Journal of Geology, Vol. III, p. 935

[5] Cf. American Naturalist, 1896, p. 128.

[6] Cf. American Naturalist, 1895, p. 567.

etic relations between igneous rocks can properly mark the lines along which a systematic classification of them may be established.

Petrographical Notes.—In a phyllite-schist found in blocks on the south shore of Lake Michigamme in Michigan, Hobbs[7] has discovered large crystals of a chloritoid like that described by Lane, Keller and Sharpless in 1891. The rock in which the crystals occur is a mass of colorless mica scales through which are distributed large flakes of biotite, small blades of chloritoid, a few acicular crystals of tourmaline and grains of magnetite. Most of the chloritoid is in large porphyritic crystals imbedded in this matrix. The optical properties of the mineral correspond to those of masonite.

In a summary of the results of this work in the upper Odenwald Chelius announces the existence there of two granites—the younger a fine grained aplitic variety and the older a coarse grained porphyritic variety, with a parallel structure due to flowage. Pegmatitic veins that cut this granite are looked upon as linear accumulations of porphyritic feldspar crystals. Many notes are also given on the diorites, gabbros and basalts of the Odenwald, on the basic enclosures in the granite, which the author regards as altered fragments of foreign basic rocks, but nothing of a startling nature with reference to these subjects is recorded. A gabbro porphyry was found occurring as a dyke mass. It consists of phenocrysts of labradorite in a gabbro-aplitic groundmass.

In a general paper on the divisibility of the Laurentian in the Morin area N. W. of Montreal, Canada, Adams[8] describes the characteristics of the members of the Grenville series of gneisses, quartzites and limestones. The augen gneisses, the thinly foliated gneisses and the granulites of the series are all cataclastic or granulitic in structure. They are regarded as squeezed igneous rocks. The crystalline limestones and quartzites are recrystallized rocks that are thought to be changed sedimentaries. Pyroxene gneisses, pyroxene granulites and other allied rocks are of doubtful origin. In addition to all these rocks there is present in the series a group of peculiar banded garnetiferous gneisses which from their chemical composition are regarded as in all probability metamorphosed sedimentary rocks.

[7] Amer. Jour. Sci., Vol. L, 1895, p. 125.
[8] Amer. Jour. Sci., Vol. L, 1895, p. 58.

GEOLOGY AND PALEONTOLOGY.

The Paleozoic Reptilian Order Cotylosauria.—A paper was read before the American Philosophical Society, November 15, 1895,[1] by Prof. E. D. Cope, on the reptilian order Cotylosauria. The following is an abstract of the characters of the order.

Quadrate bone united by suture with the adjacent elements. Temporal fossa overroofed by the following elements: Postfrontal, postorbital, jugal, supramastoid, supratemporal, quadratojugal. Tabular bone present. Vertebræ amphicoelous; ribs one headed. Episternum present. Pelvis without obturator foramen.

This order is of great importance to the phylogeny of the amniote Vertebrata. The structure of the temporal roof is essentially that of the Stegocephalous Batrachia, while the various postorbital bars of the amniote Vertebrata are explained by reference to the same part of its structure.

The palatal elements in this order are more or less in contact on the middle line, and the pterygoids diverge abruptly from this point, and return to the quadrate. The occipital condyle is single, and does not include exoccipital elements (unknown in Elginia).

Intercentra are present in Pariasauridæ, Diadectidæ and Pariotichidæ, and they are wanting in Elginiidæ. The hyposphen-hypanterum articulation is present in the Diadectidæ, but is wanting in the Elginiidæ and Pariasauridæ.

The scapular arch is best known in Pariotichidæ, Pariasauridæ[2] and Diadectidæ. In the two former there is a T-shaped episternum, over which are applied the median extremities of the clavicles; and there are well-developed coracoid and praecoracoid. In Diadectidæ[3] (probably genus Empedias) the episternum is articulated by suture with the clavicles.

In the Proceedings of the American Philosophical Society, 1892, p. 279, in a paper on "The Phylogeny of the Vertebrata," I wrote as follows: "Moreover, the Pelycosauria and the Procolophonina have the interclavicle, which is an element of membranous origin, while in the Prototheria we have the corresponding cartilage bone, the episternum. This element is present in the Permian order of the Cotylo-

[1] See Proceedings Amer. Philos. Soc., Vol. XXXIV, 1896, p. 436.

[2] Seeley, Philos. Trans. Roy. Soc. London, 1888, p. 89; 1892, p. 334.

[3] Cope, Proceeds. Amer. Philos. Soc., 1883, p. 635.

sauria which is nearly related to the Pelycosauria." The examination of the sternal region in Pariotichus has led me to the conclusion that the episternum and interclavicle are present and fused together in that genus, and also to the belief that the episternum is present in the genus Procolophon. The structure is generally similar in the two genera, and I think that Seeley is in error in determining the element in question in Procolophon as the interclavicle only.[4] Gegenbaur pointed out in his *Comparative Anatomy* the different (*i. e.*, membranous) origin of the interclavicle of the Lacertilia, but he included it with the episternum under the same name. The true episternum is not present in the Lacertilia. It is present in the Sauropterygia and Testudinata and probably in all the orders with one postorbital bar, or Synaptosauria, while it is wanting in most or all of the Archosaurian series, and in the Squamata. Whether the element I have referred to in the genus Naosaurus as interclavicle, is that element or the episternum, must remain uncertain until I can see it in place. Its edges are thin, as in the interclavicle of the Lacertilia. Of course, the Reptilian order which is in the line of ancestry in the Mammalian will have an episternum and not an interclavicle only. The Stegocephalia among Batrachia possess an episternum, with, perhaps, an adherent interclavicular layer as in the Testudinata.

Seeley describes four sacral vertebræ in Pariasaurus. In Empedias there are but two. The pelvis is without obturator foramen. The humerus has an entepicondylar foramen. The tarsal and carpal elements are incompletely known.

There are palatine teeth in Empedias and Pariasaurus, but none in Elginia; vomerine teeth none.

The inferior surface of the cranium is known in Elginia, Pariasaurus, Empedias and Pariotichus, and has been described as to the first three genera by Newton, Seeley, and myself. Pariotichus displays generally similar characters. There is a pair of posterior nares, and a pair of zygomatic foramina, but no palatine foramen. The palatine elements meet on the middle line, but gape behind. The vomers (prepalatines) are distinct, and are well developed anterior to the palatines. The ectopterygoid is large and has a prominent posterior border. I have stated that in Empedias there are teeth on the vomer. Better preserved specimens of Pariotichus show that the teeth are really borne on the edges of the palatines, which are appressed on the median line in the former genus. Similar palatine teeth are present in Pariasaurus, but are wanting in Elginia. Teeth are also present on the posterior

[4] Philos. Transac. Royal Society, 1889, p. 275, Pl. IX, fig. 9.

edge of the ectopterygoids in Pariasaurus and Pariotichus, but not in Elginia or Empedias. A character of the American genera is the weakness of the attachment of the basioccipital to the sphenoid. The basioccipital is lost from the only known specimen of Elginia, and the sphenoid projects freely below it in Pariasaurus, The roof of the mouth in this order is a good deal like that of the Lacertilia, lacking the palatine foramen.

The order Cotylosauria was defined by me in the AMERICAN NATURALIST for 1880, p. 304, and in 1889 (October). In 1889 (Transac. Roy. Soc. London, p. 292), Prof. Seeley gave it the name Pariasauria. In my *Syllabus of Lectures on Vertebrate Paleontology* (1891, p. 38), I arranged the group as a suborder of the Theromora. In 1892 (*Trans. Amer. Philos. Soc.*, p. 13, Pl. I), I again regarded the Cotylosauria as an order, and described the characters of the skull in three of the genera, and gave figures of them.

Seeley has objected to the reference of the genera Pariasaurus and Empedias to the same order, on the ground that the elements connecting the supraoccipital and the quadrate rest on the occipital elements in the latter, while they are elevated above them in the former. This character would not, however, define orders, as both conditions are found in Lacertilia; but might distinguish families within an order. However, Seeley's description and figure of the occipital region in *Pariasaurus bainii*[5] show that the structure only differs from that of the Diadectidae in the presence of a large foramen between the supraoccipital and exoccipital bones on each side.

The known species of the Cotylosauria range in dimensions from that of the South American Caimans (Chilonyx, Pariasaurus sp.) to that of the smaller Lacertilia, e. g., *Eumeces quinquelineatus* (Isodectes and Pariotichus sp.). They range from the Coal Measures to the Trias, inclusive, and have been found in South Africa, North America and Scotland. A single genus has been found in the Coal Measures of Ohio, which is represented by a species which I called *Tuditanus punctulatus*.[6] It is of small size, and as the maxillary teeth are of equal length, I cannot distinguish it from Isodectes, which belongs to the Pariotichidæ. The other species which were referred to Tuditanus are Stegocephalia.[7] *This is the first identification of a true reptile in the Coal Measures.*

[5] Philos. Transac. Roy. Soc. 1892, p. 326, Pl. XVIII, Fig. 2.

[6] Transac. Amer. Philosoph. Society, April, 1874, separate p. 11. Report Geol. Survey of Ohio, 1875, Paleontology, p. 302, Plate XXIV, fig. 1 (erroneously named in explanation *Tuditanus longipes*).

[7] Proceeds. Amer. Philos. Soc., 1871, p. 177.

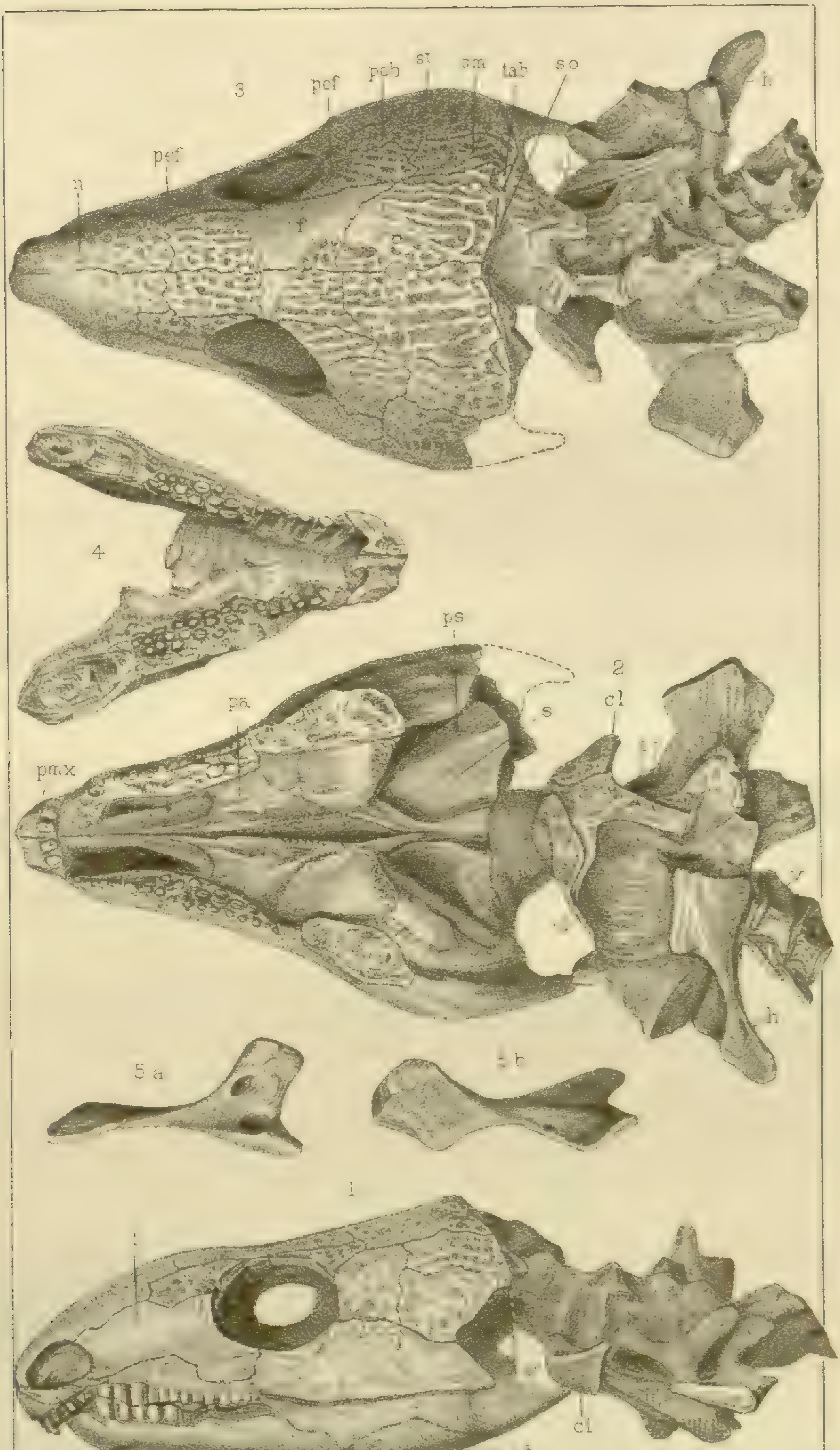
This order embraces, at present, four families, comprising 24 species distributed among 12 genera, as follows: Elginiidae, 1 genus, 1 species; Pariasauridæ, 3 genera, 7 species; Diadectidæ, 5 genera, 9 species; Pariotichidæ 6 genera (of which 3 are new, viz.: Isodectes, Captorhinus and Hypopnous), and 12 species, of which 5 are new. Total, 29 species, 15 genera.—E. D. Cope.

Explanation of Plate VIIa.

Pariotichus aguti Cope. From the Proceeding Amer. Philos. Society, November, 1895. Fig. 1, Skull, from side. Fig. 2, Skull, with angular parts of mandible adherent, cervical vertebrae and scapular arch, from below. Fig. 3, Skull, from above, with cervical vertebrae. Fig. 4, Anterior two-thirds of mandibular arch, with adherent premaxillary bones, from above. Fig. 5, Humerus. *N.*, Nasal bone; *F.*, Frontal; *Pef.*, Prefrontal; *Pof.*, Postfrontal; *P.*, Parietal; *Pmx.*, Premaxillary; *Mx.*, Maxillary; *J.*, Jugal; *Qj.*, Quadratojugal; *St.*, Supratemporal; *Sm.*, Supramastoid; *Tab.*, Tabulare; *So.*, Supraoccipital; *V.*, Vomer; *Pa.*, Palatine; *Par.*, Paroccipital; *Ecp.*, Ectopterygoid; *Ps.*, Pterygoid; *Q.*, Quadrate; *Ce.* Clavicle; *Ep.*, Episternum; *H.*, Humerus.

The Puget Group.—Sir Wm. Dawson confirms the opinion advanced by Dr. G. M. Dawson in 1890 that the formation in the north-western part of the United States to which the name Puget group has been given, extends into British Columbia as far as Burrard's Inlet. This great estuarine deposit extends southward as far as the Columbia River and from the coast line to the Cascade range, within which its beds rise to a height estimated at from 800 to 5000 feet above the level of the sea. They overlie the Cretaceous Chico series in the United States, and its equivalent the Nanaimo formation in Canada. The latest views of paleobotanists and geologists of the United States seem to be that these beds are of Eocene age and that the fossil plants may be best compared with those of the Upper Laramie of the interior plains. In so far as Canada is concerned it has been established that the Upper Laramie beds underlie a formation containing animal fossils of the White River Miocene period, so there can be no doubt as to their Eocene age, and consequently of the Eocene age of the Puget group in Canada.

A further confirmation as to this view of the age of the formation in question is found in a collection of fossil plants from the vicinity of Burrard Inlet. These were referred to Sir Wm. Dawson for identification who sums up the results of his study as follows:



PARIOTICHUS AGUTI COPE.

"A comparison with the flora of the Upper Cretaceous Nanaimo series shows that the Burrard Inlet species are distinct and of more modern aspect. On the other hand, they are also distinct from those of older Miocene deposits of the Similkamen district and other parts of the interior of British Columbia. Between these they occupy an intermediate position; in this resect corresponding with the Laramie of the interior plains east of the Rocky Mountains. They also resemble this formation in the general facies of the flora, which is not dissimilar from that of the Upper Laramie or Fort Union group."

"We may thus refer these plants to the Paleocene or Eocene, and regard them as corresponding with those of the Atanékerdluk beds in Greenland, the lignitic series of the McKenzie River, and the beds holding similar plants in Alaska."

"This flora thus serves to fill the gap in our western series of fossil plants, namely, that between the Cretaceous and the Lower Miocene." (Trans. Roy. Soc. Can. (2), Vol. I, 1895–'96.)

The Geological Structure of Florida is according to Prof. E. T. Cox, remarkable for its simplicity. The underlying rock is a soft limestone of Upper Eocene age; resting on this are beds of phosphate of lime; and covering the phosphate and limestone is a bed of sand that varies from a few inches to 20 feet and more in depth.

The Eocene limestone is filled with fossil marine shells. It shows no evidence of disturbance and is without a trace of stratification. It has an amorphous structure and is of unknown thickness. The phosphate of lime occurs in detached masses scattered over an area about 20 miles wide, and exending in a belt, follows in general way the trend of the Gulf coast from the northern limits of the state and beyond, to the western edge of the Everglades on the south. The author believes the phosphate to be the result of the mineralization of guano.

The covering of sand is found all over the Peninsula. It has been blown by the winds from the gulf and ocean beaches. Mixed with the sand is clay in the form of fine dust. In several localities the associated clay has been separated from the sand by running water and deposited as kaolin. This kaolin has been tested and found to be of superior quality for the manufacture of the finest porcelain.

Florida is not a level plain. A ridge from 30 to 50 miles wide extends from the northern part of the state to the Everglades, having an elevation of more than 230 feet in some places. From this ridge the land slopes to the Atlantic on the east and the Gulf on the west.

The elevation of the Peninsula was due to that continental force, extended over a vast period of time, which brought the tops of the Rocky

Mountains above the waters of the Pacific. (Trans. Amer. Inst. Mining Engineers.)

The Eocene age of part of Florida was first asserted by Prof. Eugene Smith of Alabama, and this conclusion was confirmed by paleontologic data by Prof. Heilprin, of Philadelphia. Dr. W. H. Dall subsequently delimited exactly the area of these beds with the Neocene and Plistocene beds to the south, east and north of them.

Notes on the fossil Mammalia of Europe Pt. II.—*On the affinities of the Genus Tapirulus, Gervais.*—Tapirulus is one of those aberrant types, where we find a curious assemblage of characters, which to the systematist is a great annoyance, although to the morphologist most instructive.

A superficial examination of the teeth has lead some palæontologists to assign this genus a position near the Tapir. Gervais[1] established this genus on the characters of the lower true molars.[2] He referred *Tapirulus* to the family *Anoplotheriidæ,* which I shall endeavor to prove is its proper position, although this reference on his part I believe was accidental, as he placed in the same family the genus *Adapis.* Gaudry[3] has assigned *Tapirulus* a position near the genus *Tapirus,* and Zittel[4] referred it to the *Suidæ.*

Through the great kindness of Prof. Albert Gaudry, who has so generously allowed me to study so many of the beautiful specimens in the collection of the Jardin des Plantes, I have had the opportunity of examining a skull of *Tapirulus,* in which the greater part of the upper dentition is preserved. On examination of this skull I was at once struck by its close resemblance to that of *Anoplotherium,* and *Dacrytherium.*

The skull in *Tapirulus* is slender and much elongated, the dorsal contour is nearly straight, and the facial portion is strongly compressed and slender. There is no preorbital fossa, as in *Dacrytherium,* and the occiput is high and narrow, like that of *Anoplotherium.* The auditory region very closely resembles that of *Dacrytherium,* the paroccipital process is long, slender and the posttympanic and glenoid processes are applied closely to the external auditory meatus. The brain case in the *Anoplotheriidæ* is much extended anteroposteriorly, being about one-half the total length of the skull in *Dacrytherium.* In *Tapirulus* the

[1] Comptes Rend. Acad. Sci., 1850, p. 604.
[2] Zoologie et Palæontologie Francaise, p. 173, pl.
[3] Journal de Zoologie, XIV, 1875, p. 5.
[4] Traité de Palæontologie, 1894, p. 338.

cranial portion of the skull is shorter relatively than in *Dacrytherium* and with this decrease in length I observe a greater development of the anterior part of the brain case, which encloses the frontal lobes of the brain. In short the brain of *Tapirulus* as compared with the size of the skull, must have been much larger than in *Dacrytherium*, and this greater development effected especially the frontal region of the brain.

As the Suilline type of skull was well established in the Phosphorites or Upper-Eocene of France (*Cebochœrus*), I shall compare the cranium of *Tapirulus* with that of *Cebochœrus*. In the latter genus the brain case is very much reduced with the extremely prominent zygomatic arches; sagittal and lambdoidal crests are very heavy and the occiput is broader than high. The cranial portion of the skull in *Cebochœrus* is much heavier and broader than the facial. All these cranial characters in *Cebochœrus* differ decidedly from those of *Tapirulus* and I enumerate them, so as to prove that *Tapirulus* has no direct affinity with the Suillines. The skull of *Tapirulus* somewhat resembles that of the primitive Selenodonts, (*Cœnotherium*), but it is more slender and its general form more closely resembles that of the *Anoplotheridæ*.

It is, however, the peculiar structure of the teeth, which is the most important consideration in studying *Tapirulus*. It is upon the characters of the teeth that *Tapirulus* has been assigned its various positions in the Ungulata. On a superficial examination of the upper true molars of *Tapirulus*, they exhibit a certain resemblance to those of the Tapir, but studied in detail I shall endeavor to prove that the molars of *Tapirulus* differ fundamentally in their plan from any of the known Lophiodonts. In the first place the external lobes of the superior molars in *Tapirulus* are concave, and not convex as in the true Tapirine molar. Again the transverse crests are straighter and their relation to the external lobes differ from those of the Tapirs tooth. At the junction of the transverse crests and the external lobes there is a strong notch, and in one specimen I can detect a faint trace of the intermediate tubercles.

The superior molars of the *Anoplotheriidæ*. as is well known, have deeply concave external lobes, the protoconule is distinct from the protocone, the latter element being in its primitive bunodont condition. In *Anoplotherium* and *Dacrytherium* the hypocone is selenodont in structure. In other words the form of superior molar found in the *Anoplotheridæ* is a slight modification of the buno-selenodont type, it differs from the exact form of this type in having the hypocone crescentoid.

Now in *Tapirulus* the two external concave lobes of the superior molars have nearly the same structure as those of *Dacrytherium*, and the internal primitive bunodont elements of the crown, have been transformed into crests, this occurred before the *Anoplotherium* type of molar had reached its present stage of evolution. It is then not difficult to derive the form of superior molar found in *Tapirulus* from the true buno-selenodont type, and I believe this to have been the origin of the peculiarly modified molar occurring in *Tapirulus*.

The superior and inferior premolars in *Tapirulus* have the same elongated form so characteristic of the *Anoplotheriidæ* in general, and like the latter group, the canines were not differentiated in form from the anterior premolars. The lower true molars of *Tapirulus* depart widely in structure from those found in any of the known genera of the *Anoplotheridæ*, although like the upper molars they are much specialized and can be derived from a less modified type, which was the common stock of both *Tapirulus* and *Anoplotherium*. A peculiarity in the structure of the lower molars of *Tapirulus* is the presence on each tooth of a well developed third lobe, hypoconulid, which projects posteriorly a considerable distance. The portion of the molar crown anterior to the hypoconulid consists of two high transverse crests, the antero-external termination of the front crest exhibiting a rudiment of the anterior spur, which is so largely developed in all the other genera of the *Anoplotheridæ*. The lower jaw in *Tapirulus* is long and extremely slender and its form closely resembles that of *Dacrytherium*.

In conclusion I believe the natural position of *Tapirulus* is in a subfamily of the *Anoplotheriidæ*, and that both *Tapirulus* and *Anoplotherium* have been derived from a common stock, the ancestral form having had the pure type of buno-selenodont molar. As already shown above, the resemblance in structure of the molars of *Tapirulus* to that of the Tapir is not exact, the Tapirs tooth having been derived direct from the bunodont type (See *Euprotogonia* and *Systemodon*).

III.

On the validity, and systematic position of Mixtotherium Filhol.—The genus *Mixtotherium*[5] is another interesting form, not as aberrant in its characters as *Tapirulus*, but which unites in a certain degree the Suillines and the Anoplotheroids. Prof. Zittel in his "Traité de Palæontologie" does not consider *Mixtotherium* as a good genus, and refers it to the milk dentition of *Diplobune*. I am quite confident that Prof. Zittel is mistaken in this determination, and I shall endeavor to

[5] Mém. quelques Mam. fossiles, Toulouse, 1882.

prove that *Mixtotherium* is a valid genus and entirely distinct from either *Diplobune* or *Dacrytherium*.

The genus *Adiotherium* was described by Filhol in 1884. This genus is referred by Zittel to the milk dentition of *Dacrytherium*, I can not agree with Prof. Zittel on this reference either, as I believe *Adiotherium* to have been based upon the milk dentition of *Mixtotherium*.

The skull in *Mixtotherium* is essentially Suilline, but exhibiting some characters like those of the Anoplotheroids. The form of the brain case is longer and narrower than in *Cebochœrus*, and it closely resembles that of *Acotherulum* which is one of the most primitive of the early Suillines of Europe. The occipital region of the skull in *Mixtotherium* is much broader than high, and is not constricted in the middle as in *Dacrytherium*; the occiput has nearly the exact form of *Acotherulum* and *Cebochœrus*.

In the primitive pigs of the Phosphorites the auditory bullæ are extremely small, in *Mixtotherium* they are large. The basioccipital region of the skull in *Mixtotherium* is rather long and narrow, and like that of *Dacrytherium*. In *Cebochœrus* of the Phosphorites, the peculiarly elongated and constricted snout of the pigs is well differentiated, however, in *Mixtotherium* as well as *Acotherulum* the facial region of the skull is broader and shorter, its form being more as in *Dacrytherium*. *Mixtotherium* agrees with *Diplobune* and differs from *Dacrytherium* in lacking a preorbital fossa in the maxillary bone. The general form and proportions of the skull in *Mixtotherium* is very much like that of the peculiar American genus, Oreodon.

The dentition of *Mixtotherium* resembles that of the *Anoplotheridæ* in the absence of any diastemas, it differs, however, from this family in the large size of the canines, which in form resembles more those of the Suillines. The superior premolars are normal in form, and not elongated as in the Anoplotheroids. The last upper premolar closely resembles in structure a true molar, it has two external cusps, which are intermediate in structure between the bunoid and selenoid forms. The deuteroconid forms a crest with the antero-intermediate tubercle, the tetastoconid is present, but small and bunoid in structure. The structure of the superior molars of *Mixtotherium* differ from those of *Diplobune* and *Dacrytherium* in the following details; the external crescents are united externally by a prominent mesostyle, which is more constricted than in *Diplobune*; in *Dacrytherium* this portion of the molar is open widely internally.

In the *Anoplotheridæ* the protocone is distinct from the protoconule, whereas in *Mixtotherium* these elements are united and form a well

developed protoloph. In *Mixtotherium* the hypocone is selenoid in structure as in *Dacrytherium,* but this cusp is much smaller and it is much less extended internally than in that genus. I emphasize especially the large development of the mesostyle, and the presence of a protoloph, characters of the upper molars of *Mixtotherium* which differs decidedly from those of *Dacrytherium.* The structure of the fourth upper premolar in *Mixtotherium* resembles somewhat that of *Agriochœrus,* but differs from this genus in the presence of the postero-internal cusp. In *Dichodon* Owen, the complication of the fourth upper premolar is carried still further than in *Mixtotherium,* as in *Dichodon* this tooth is completely molariform and selenodont in structure. However, I believe, that *Mixtotherium* has no close affinity with *Dichodon,* as the structure of the skull and dentition in *Dichodon* is quite modernized.

The lower jaw in *Mixtotherium* is rather short and deep below the last lower molar, these characters differ strikingly from those of the *Anoplotheriidæ,* where the jaw is very slender and elongated. The mandibulæ are strongly ankylosed at the symphysis as in the primitive pigs, *Acotherulum* and *Cebochœrus,* this is a character I believe seldom found in the Mammalia outside of the Primates. The last lower premolar in *Mixtotherium* is intermediate in structure between a last milk tooth and permanent molar. It consists of an antero-median cusp, bunoid in form, and posterior to it, of two external crescents and two flattened internal elements. The structure of the inferior true molars is like that of *Dacrytherium.*

It appears to me that the genus *Mixtotherium* is of importance phylogenetically, and demonstrates how closely the Suillines and Anoplotheroids are related. In the characters of the skull and the large development of the canines *Mixtotherium* is more like the pigs, but showing affinities to the Anoplotheroids in the form of the brain case. The structure of the molars, as already shown, resemble very closely those of the *Anoplotheriidæ* and have gone one step further in their specialization by the development of a well defined protoloph.

Schlosser in his paper, "Stammesgeschichte der Hufthiere" speaking of the origin of the Sullines remarks "die Herkunft diese Stammes ist noch in vollständiges Dunkel gehüllt, nur so viel dürfen wir als sicher annehmen, dass derselbe wohl von der gleichen Grundform ausgegangen ist wie der der Suiden." The Oreodonts are considered by Scott to be related to the Anoplotheroids, and if this be the case it is not strange that the skull of *Mixtotherium* resembles that of *Oreodon.* The genus *Protoreodon* of the Uinta or Upper Eocene, has the five lobed superior molar typical of the Anoplotheroids, and the primitive Suillines.

In conclusion, *Mixtotherium* is then a type intermediate between the Suillines and the Anoplotheroids, and has been derived from a common stock, which also gave origin very probably to the Oreodonts.—CHARLES EARLE, Laboratoire de Palæontologie, Jardin des Plantes, Paris.

The Glaciers of Greenland.—Prof. Chamberlin's report on the Geology of Greenland contains the results of his observations of glacier phenomena in the region explored by the Peary auxillary expedition of 1894. The seventeen glaciers visited fall into two classes designated the southern and northern types. The former are distinguished by ending in a slope of moderate declivity, the latter end in abrupt terminal walls which rise to heights of 50 to 150 feet. The author notes here that he is speaking of glaciers that end upon the land. Obviously, those that reach the sea terminate in vertical walls through the breaking away of the ends. Not only are the ends of the glacial tongues vertical, but in some instances the sides are so likewise. To some extent the edge of the ice-cap itself is vertical.

The stratification of these glaciers is remarkable for extent and definiteness. The ice is almost as distinctly bedded as sedimentary rock. The following points are noted by the author:

" In the vertical face there are usually presented two distinct divisions, an upper one of nearly white ice, whose laminations are not conspicuous, from lack of differential coloration, and a lower one discolored by debris, which gives great distinctness to the bedded structure. The lower divisions is divided by very numerous partings, along which are distributed rocky debris, embracing not only sand and silt, but rubble and boulders. Often the amount of this interspread debris is so sligbt as to constitute the merest film, while at other times it reaches a thickness of an inch or two. . . . . . In general, the rocky debris is arranged in very definite and limited horizons leaving the ice above and below as clean and pure as any other. It is very notable and significant that the ice next the debris layers is the firmest and most perfect that the glacier affords. The coarser debris is arranged in the same horizons with the fine silt and clay. . . . . . Where ice is well laminated, as it commonly is, the laminations bend under and over the embedded boulders. This seems to indicate that the embedded boulders do not descend through the ice by virtue of superior gravity, but are retained in the original position given them by the embedding process. The extent to which the basal portion of the ice is laminated is remarkable. In selected cases twenty laminations might be counted to the

inch. These laminations are sometimes symmetrical, straight and parallel. At other times they are undulatory, and in instances they are greatly curved and contorted in an intricate fashion."

It was observed that the debris bearing layers were parallel to the base of the glacier and were confined to its lower 50 or 75 feet, with some few exceptions. Even at the border of the glacier clean layers of white ice above the debris strata constituted one-third or more of the section. This is contrary to the view that the debris habitually works up to the surface and forms a layer there as it nears the border of the glacier.

Prof. Chamberlain was fortunate in being able to observe the process of introduction of debris in progress. At a point in the Gable glacier there was found an embossment of rock over which the ice was forced to pass and in so doing to rise in a dome-like fashion. One side of the dome was melted away, revealing operations at its base. Combining a number of observations, the author gives the following interpretation of the process:

"The bottom layer of the ice in passing over the crest of the embossment would be pressed with exceptional force upon it, and would, as a result, be especially liable to detach fragments from it and imbed them within itself. If debris were being pushed or dragged along between the ice and the rock surface beneath, it would be pressed into the ice and the ice compacted about it with exceptional force. As any given portion of the basal layer passed beyond the crest of the embossment, the vertical pressure would tend to cause it to follow down the lee slope, while the horizontal thrust of the moving ice would tend to force it straight forward. If any given portion yielded to the first and passed down the slope, it would produce a curve in the hardened basal layer of ice. As a result of this, the horizontal thrust, instead of continuing to act along the disadvantageous curved line, and against the superior friction of the bottom, would be disposed to cause the layer to buckle at the bend. The fold so formed would be elongated and appressed by the continuation of the process and become a layer. The ice, beneath, however, would gradually yield, and the debris layer would settle down out of the line of maximum thrust and the conditions for a new fold be induced."

Cases of true faulting and overthrust were seen, the rocky debris being carried along the fault plane.

As to the method of movement, Prof. Chamberlin presents evidence, which taken in connection with the intrusion and interstratification of earthy material, would seem to indicate that these glaciers move, in some notable part at least, by the sliding of one layer upon another.

Several instances were noted where the glaciers had advanced over their terminal moraines by riding up over them, but none where the ice showed any competency to push the frontal material, even its own debris, before it.

A driftless area was discovered on the east side of Bowdoin Bay immediately adjoining the present great ice-cap. (Bull. Geol. Club, Phila., 1895.)

BOTANY.[1]

New Species of Fungi.—The activity of our fungologists is indicated by the long lists before us which have been published within the last few months. From the Proceedings of the Academy of Natural Sciences of Philadelphia (1895, pp. 413 to 441) we have "New Species of Fungi from Various Localities," by J. B. Ellis and B. M. Everhart, including ninety-nine species. Many of these are from Colorado and other western regions. We note among the more interesting species the following, viz.: *Fomes alboluteus*, from an altitude of 10,000 feet, in Colorado; *Bovista cellulosa*, *Lycoperdon alpigenum*, both from Colorado, the latter from an altitude of 11,500 feet; *Rosellinia geasteroides* from Louisiana; *Phyllachora plantaginis*, parasitic on *Plantago rugelii*, in Wisconsin.

The same authors publish in the October (1895) *Bulletin of the Torrey Botanical Club*, a paper on "New Species of Fungi" in which there are described eight new species from the Sandwich Islands, eleven from Florida, and six from Mexico. It is with much pleasure that we observe that but two of the specific names are dedicated to persons, viz.: *Schizophyllum egelingianum* (probably a synonym for *S. commune*) and *Melogramma egelingii* from Mexico. It is to be hoped that the good example here set may be followed by others upon whom it falls to find names for new species.

In the Fourth Report of the Botanical Survey of Nebraska, just issued, fifty-five new species of fungi are described by Roscoe Pound, F. E. Clements and C. L. Shear. These are distributed as follows: in the *Mucoraceæ*, 1; *Sphærioideæ*, 1; *Mucedinaceæ*, 2; *Dematiaceæ*, 2; *Stilbaceæ*, 1 (in the new genus *Trichurus* of Clements and Shear); *Tuberculariaceæ*, 2; *Helvellaceæ*, 2, *Pezizaceæ*, 24; *Bulgariaceæ*, 1; *Agari-*

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

caceæ, 20. In the last named family the name *Gymnochilus* is substituted for *Psathyra* of Fries (1821) which must fall, since it is identical with Commerson's *Psathura* (Juss. Gen. ,1789). In the *Pezizaceæ* the name *Lachuea* Fries (1822), being identical with *Lachuæa* L. (Sp. Pl., 1753), must give way to *Sepultaria* Cooke (1879).—CHARLES E. BESSEY.

Alaskan Botany.—In the Contributions from the U. S. National Herbarium (Vol. III, No. 6), F. V. Coville makes a report upon the collections of plants made on Yakutat Bay, Alaska, in 1892, by Frederick Funston. Mr. Coville's paper is preceded by a Field Report made by Mr. Funston. The latter contains much interesting information as to the country and its vegetation. In regard to the latter the author says, "The plant life of the region about Yakutat Bay is characterized by the dense and vigorous growth of a comparatively small number of species, giving the forests especially an appearance of great sameness. The almost level country lying on the eastern side of the bay, between Ocean Cape and the foothills of the mountains, is covered with a forest growth practically impenetrable. The great amount of fallen timber, together with the tangled and heavy undergrowth constitute such obstacles to travel that even the Indians who have lived here many years have never penetrated the forests of the mainland for a mile from their own village. The great bulk of this forest is composed of the Sitka Spruce (*Picea sitchensis*), which in this region reaches a height of seventy feet. This tree extends from sea level to an altitude of 2,200 feet on the sides of Mt. Tebenkof; but as one follows the coast line up the bay from this mountain, the upper limit becomes lower and lower, until at the entrance of Disenchantment Bay it reaches sea level, the tree not being found on the shore of this bay. A large forest lies along Dalton Creek, and there are several of considerable extent between this place and Point Manby."

"The timber of the spruce tree plays a most important part in the economy of the natives, as from it are constructed their houses and canoes, and it is used in the manufacture of oil crates, bows, arrows and other implements, while the smaller roots after being boiled and split are used in basket weaving."

The other woody plants mentioned are the hemlock (*Tsuga mertensiana*), Sitka cypress (*Chamæcyparis nootkatensis*), red alder (*Alnus rubra*), a willow (*Salix barclayi*), the elder (*Sambucus racemosa*), the Menziesia (*Menziesia ferruginea*), high bush cranberry (*Viburuum pauciflorum*), the blueberry (*Vaccinium ovalifolium*), salmon berry

(*Rubus spectabilis*), devil's club (*Echinopanax horridum*), and black currant (*Ribes laxiflorum*).

The catalogue of species includes 159 species, of which 122 are Anthophytes; 3, Gymnosperms; 9, Pteridophytes; 25, Bryophytes. The ten largest families are as follows: *Rosaceæ*, 13 species; *Carduaceæ* (*Compositæ*), 10; *Poaceæ* (*Gramineæ*), 10; *Ranunculaceæ*, 9; *Saxifragaceæ*, 9; *Scrophulariaceæ*, 8; *Ericaceæ*, 7; *Polypodiaceæ*, 6; *Ammiaceæ* (*Umbelliferæ*), 6; *Brassicaceæ* (*Cruciferæ*), 5.—CHARLES E. BESSEY.

Aquatic Plants of Iowa.—R. I. Cratty has published in the Bulletin of the Laboratories of Natural Science of the University of Iowa (Vol. III, No. 4) some "Notes on the Aquatic Phenogams of Iowa," which will be useful in recording the past and present distribution of plants which are fast disappearing. It is a pity that author and editor permitted the antiquated spelling of Phanerogam to be used. There can be no valid excuse for "Phenogam." The species noted are *Arisaema triphyllum*, *A. dracontium*, *Symplocarpus fœtidus*, *Acorus calamus*, *Lemna minor*, *L. trisulca*, *L. polyrrhiza*, *Wolffia braziliensis*, *Typha latifolia*, *Sparganium simplex*, *S. androcladum*, *S. eurycarpum*, *Naias flexilis*, *Zannichilia palustris*, *Potamogeton natans*, *P. amplifolius*, *P. nuttallii*, *P. lonchitis*, *P. heterophyllus*, *P. illinoensis*, *P. prælongus*, *P. perfoliatus*, *P. zosteræfolius*, *P. foliosus*, *P. major*, *P. pusillus*, *P. spirillus*, *P. pectinatus*, *Triglochin maritima*, *Scheuchzeria palustris*, *Alisma plantago*, *Echinodorus rostratus*, *E. parvulus*, *Sagittaria arifolia*, *S. latifolia*, *S. rigida*, *S. graminea*, *S. cristata*.—CHARLES E. BESSEY.

Another Elementary Botany.—Professor MacBride has recently brought out a little book on botany for secondary schools, under the title of "Lessons in Elementary Botany," issued by the house of Allyn and Bacon of Boston. The author presents in small space essentially that phase of botany with which we have long been familiar in Gray's "Lessons," Miss Youmans's "First Book," "Second Book" and "Descriptive Botany," and Wood and Steele's "Fourteen Weeks in Botany." Whatever merits and demerits these works have are here reproduced, somewhat modified of course. The lessons begin with "buds," followed by "stems," "roots," "the leaf," "inflorescence," "the flower," "the fruit and seed." These topics occupy about eighty-five pages, and while the subject matter is essentially similar to that in Gray's "Lessons," the treatment resembles that of Youmans's books, considerably simplified. The pupil is required to work out the

details of structure by actual examination. The remainder of the body of the book (pp. 85 to 207) is taken up with selected plants whose structure is to be worked out, and here the treatment reminds one of Wood and Steele's book and the corresponding chapters in Miss Youmans's earlier books. There is here, however, a considerable improvement in the presentation of the matter, the pupil being led on by questions which direct attention to different details.

A valuable part of the book is found in the appendix, where directions are given for collecting and preserving materials for study. Taken altogether, the book is a good one, although we cannot agree with the author that gross anatomy alone, and that practically confined to the flowering plants, is all that can be done in the secondary schools. We prefer the work suggested by the Natural History Conference, as reported by the Committee of Ten, and know from much personal experience that the high schools are rapidly supplying themselves with compound microscopes, by means of which the pupils are obtaining some knowledge of the lower plants, and of the vegetable kingdom as a whole. Neither can we endorse what the author says in the preface as to the relative value to the pupil of a knowledge of the higher rather than the lower plants. But with all these criticisms it must not be thought that the book is a poor one; on the contrary, for schools where the conditions are such as the author describes and where they must so remain, the book is a very good one.—CHARLES E. BESSEY.

Botany in the United States Department of Agriculture.—From the recent Report of the Secretary of Agriculture, we glean the followings items, relating to the work in botany. Investigations for determining the strength of timbers of various species have been continued in the Division of Forestry, no less than 13,000 tests having been made during the year preceding the report. Measurements upon a large scale of the rate of growth of pine trees have been begun and some preliminary results obtained. Under this head the announcement is made of the establishment of experimental plantings at several points upon the Great Plains. In the Division of Botany the following announcement is gratifying to botanists. "The herbarium of the Department of Agriculture, commonly called the National Herbarium, having out-grown its old quarters, was, by kind permission of the Secretary of the Smithsonian Institution, removed and well installed in the fire-proof building of the National Museum, where it will be cared for by the botanists of this Department. This herbarium is steadily being built up and enlarged at the expense of the Department of Agriculture."

The new division of Agrostology, established during the current fiscal year has for its special work the scientific and economic study of the grasses and others forage plants. In connection with this work it is the purpose of the officers of the Division to establish "Experimental Grass Stations" in which the study of particular species may be more readily pursued.

The division of Vegetable Pathology "has been broadened during the year to include plant physiology," and the Secretary adds, "It is believed that this will add materially to the value of the investigations."

The abolition of the "Division of Microscopy" is announced. When first established, twenty years ago microscopy "was considered a separate branch of technology, but since that time the microscope has come into daily, almost hourly, use in nearly all scientific laboratories." The Secretary very properly concludes that a separate division is now "an absurdity."—Charles E. Bessey.

Notes on Recent Botanical Publications.—From the Division of Botany of the U. S. Department of Agriculture we have John M. Holzinger's "Report on a collection of Plants made by J. H. Sandberg and Assistants in Northern Idaho in the year 1892." Some new species are described, viz., *Cardamine leibergii* (figured in Plate III as *C. sandbergii*), *Peucedanum salmoniflorum*, *Dicranoweisia contermina*, *Orthotrichum holzingerii*, *Bryum sandbergii*, and *Peronospora giliæ.*—Another contribution from the same source is the "Report on Mexican Umbelliferæ, mostly from the State of Oaxaca, recently collected by C. C. Pringle and E. W. Nelson," by John M. Coulter and J. N. Rose. As was to be expected, many new species were found in the collection.—With the preceding paper is a smaller one by J. N. Rose, entitled "Descriptions of plants, mostly new, from Mexico and the United States," the new species from the United States are *Ligusticum eastwoodæ*, from the La Plata Mts., Colorado; *Vlœa glauca*, from Oregon; and *Thurovia triflora*, a curious Texan composite for which a new genus had to be erected.—From the Field Columbian Museum we have C. F. Millspaugh's "Contribution to the Flora of Yucatan," which is marked "Botanical Series, Vol. I, No. 1," of the publications of this new centre of scientific activity. It includes the results of an expedition to Yucatan made in January, 1895, to which the author has added species compiled from Hemsley's *Biologia Centrali-Americana.*—M. E. Jones's "Contributions to Western Botany," published in the Proceedings of the California Academy of

Sciences, is mainly taken up with the new species discovered by him while acting as Field Agent for the U. S. Department of Agriculture. The author says "the long delay in the publication of the report necessitates the early publication of the new species." The author does not follow the "Rochester Rules" of nomenclature, and gives some reasons for not doing so, but the reader is amused to find under *Oxytropis acutirostris* (Watson) the remark "should it be necessary to reduce this genus to *Spiesia*, the name must be *S. acutirostris* (Watson)," and again under *Oxytropis nothoxys* (Gray), the synonym *Spiesia nothoxys* (Gray). For one who does not accept the "Rochester Rules" this is indeed a remarkable proceeding, since it is the deliberate addition of two synonyms (with "Jones" as the authority) to what the author calls "the mass of new names, nine-tenths of which are wholly useless."—K. C. Davis has issued a "Key to the Woody Plants of Mower County, in Southern Minnesota, in their Winter Condition" in the form of a five-page pamphlet. It will be useful in the region for which it is intended.—An interesting paper comes from Dr. G. Clautriau of Brussels, entitled Étude Chemique du Glycogène chez les Champignous et les Levures," from which we hope to make extracts in some future number.—CHARLES E. BESSEY.

VEGETABLE PHYSIOLOGY.[1]

Ambrosia.—By this name Schmittberger designated a soft watery substance found in the burrows of certain beetles and supposed to be of use in feeding the larvæ. The exact nature of this ambrosia appears to have been for a time in doubt, owing to the fact that it was generally seen by entomologists rather than by mycologists. Of late years, however, it has been conceded to be of fungous origin, although no one appears to have studied it critically. Since the appearance of Möller's book on the Fungous Gardens of South American Ants, the subject of ambrosia has received renewed attention. In this country, Mr. Henry G. Hubbard, who has long paid especial attention to the habits of coleoptera, has repeatedly observed this substance in the chambers of *Xyleborus pubescens* in orange trees in Florida, and has recently discovered it in the burrows of *Corthylus punctatissimus* in

[1] This department is edited by Erwin F. Smith, Department of Agriculture, Washington, D. C.

the roots of whortleberry near Washington, D. C. Specimens from the latter source were submitted to various students of fungi in Washington last autumn for identification, and the writer had full opportunity to examine this substance. Some of the chambers were filled with it, others partly filled, and others free from it. It is a colorless much septate mycelium, inclined to be constricted at the septa, and in places consisting of rounded, nearly iso-diametric, colorless, rather thick-walled cells, not sufficiently differentiated from the mycelium to be considered as true spores. It appears to be the mycelial or oidial stage of some higher fungus, probably of some Ascomycete. From its distribution in the burrows and the behavior of the beetles toward it, there can be little doubt that it serves them for food. Whether like the ants they actually cultivate it, is another question and one more difficult to solve. In Germany, where this ambrosia was first discovered, Prof. R. Goethe, Director of the Royal Lehranstalt fur Obst-Wein-und Gartenbau zu Geisenheim am Rhein, has recently published an account of its discovery in the chambers of *Xyleborus dispar*. Prof. Goethe's brief note (p. 25, *Berichte d. Kgl. Lehranstalt* etc.) is accompanied by a good figure, judging from which the fungus appears to be the same as that found in the chambers of *Corthyllus punctatissimus* near Washington. This fungus is said to be the same as that found in 1883 in the burrows of *Xyleborus* in cherry trees at Kamp am Rhein. Concerning the use made of this fungus by the beetles he makes the following statement: Seine Wucherungen dienen ganz unzweiflehaft den Käfern zur Nahrung, denn man sieht deutlich, wie der Ueberzug stückweise abgeweidet wird. Further study of this subject would undoubtedly bring to light many interesting things. In the next number of the NATURALIST I hope to publish a note from Mr. Hubbard on this subject.—ERWIN F. SMITH.

White Ants as Cultivators of Fungi.—In connection with the preceding it may be worth while to reprint part of a note which appeared in *Grevillea*, June, 1874, p. 165–6, relative to the occurrence of fungi in the nests of termites in India. A writer in the *Gardeners' Chronicle* stated that he had never seen any fungi on or in nests of white ants except very small ones less than the size of a pin head. In opposition to this Mr. W. F. Gibbon, Doolha, Goruckpore, wrote to the Horticultural Society of India as follows: "I send you now a bottle containing mushrooms I extracted a few days ago from the center of a white ant hillock. When I collected them they were in appearance like asparagus, over 14 inches in length, and the people about here

consider them particularly good eating, partaking of them both raw and cooked. When I read the above article in your Society's Journal somewhat over a year ago, I was then aware that mushrooms existed in the interior of ant hills, for I had often seen them, but I did not know their season of sprouting, and whenever I searched was unsuccessful till the other day. I have now ascertained the season they sprout is the end of August or the beginning of September, and I believe all ant hills produce them. These mushrooms appear to me to proceed from a peculiar substance always found in ant hills in this country (whether white or black), generally called ants' food, a bluish gritty substance, like coarse wheat flour turned mouldy and adhesive. In dry weather brittle, and in damp weather like soft leather. It is this substance, under the combined influence of heat, damp and darkness from which the mushrooms grow. As my experience is at variance with the writer in the *Gardeners' Chronicle*, you may care to record it. * * * I would like these mushrooms, if possible, referred to some mycologist, and their names ascertained; and I would like also to know if the bluish substance, the ants' food, was collected and treated artificially, could similar mushrooms be raised." These mushrooms were submitted to Dr. D. D. Cunningham, who reported as follows: "I herewith return the letter sent to me more than a month ago, along with specimens of fungi said to have been procured from the interior of a white ant hill. The specimens apparently belong to some species of *Lepiota*, and are chiefly remarkable for the extreme length and coarse fibrous contents of the stem. The occurrence of fungi in connection with ant hills is well known, but in so far as I am aware, those hitherto described as occurring on the hills of the white ant belong to species of the Gasteromycetous order *Podaxinei*, so that the occurrence of a species of one of the sub-genera of Agaricus in such localities is a new and interesting fact. With regard to the material from which they arise, and which must apparently be of the same nature as the so-called spawn of the cultivated mushroom, consisting of vegetable debris permeated by the mycelium of the fungus, it may be noted that a similar substance is described by Belt as occurring in the nests of the leaf-cutting ants in Nicaragua, and is supposed by him to serve as food—the ants culling and storing the leaves for the sake of the fungi which are subsequently developed in the debris (*Naturalist in Nicaragua*, p. 80). Were this spawn artifically exposed to conditions similar to those which it naturally encounters in the interior of the hillocks—heat, darkness and moisture—I believe that the pilei

might very probably be raised at will, and if they really are good eating, the experiment would be well worth trying."

—Erwin F. Smith.

Desert Vegetation.—Perhaps the most interesting part of Rev. George Henslow's recent book, *The Origin of Plant Structures,* are the two chapters on desert plants. The first of these chapters is devoted to a consideration of the origin of the morphological peculiarities of desert plants; the second to the histological peculiarities of such plants. A large amount of data are brought together, rather hastily it would appear, going to show that the peculiarities of desert plants are the direct outcome of the conditions under which they grow, in other words, that these peculiar modifications, such as reduction of leaf surface, increase of succulency, acquisition of spines, development of water storage tissues, sinking of the stomata below the level of the surface, excessive development of cuticle, of wax, or of hairiness, change from annual to biennial or perennial, increased length of roots, etc., are all brought about by the direct action of environment on the plant. "Natural selection," in the author's own words, "plays no part in the origin of species." These two chapters are well worth the perusal of all who are interested in the study of the flora of our western mountains and arid plains, and the whole book will serve to provoke thought. Other chapters deal with origin of structural peculiarities of alpine and arctic plants; maritime and saline plants; phanerogamous aquatic plants, etc. The book is a companion volume to the author's *Origin of Floral Structures through Insects and other Agencies.*—Erwin F. Smith.

A Second Rafinesque.—*Die Pestkrankheiten (Infectionskrankheiten) der Kulturgewächse; Nach streng bakteriologischer Methode untersucht und in völliger Uebereinstimmung mit Robert Kochs Entdeckungen geschildert von Prof. Dr. Ernst Hallier,* is one of the querest books it was ever the lot of the writer to read. It was published at Stuttgart in 1895 by Erwin Nägale, and contains 144 8 vo. pages and 7 fairly well executed plates. Concerning this book it may be said that its author is either an undiscovered great genius or else a very crazy man. About one-third of the book is given up to caustic abuse of Anton de Bary and his students, relative to which it may be said that Dr. de Bary's reputation is safe not only in the hands of his friends but also in the hands of all who love clear thinking and honest work; and all this without defending any of the errors into which he may have fallen. Another third of the book or thereabouts is devoted to the description of old and well known species of Peronosporaceæ,

little that is really new being added, but most facts being correctly stated. The names of many of the species, however, are changed for reasons which would not be recognized as good even by the most ultra radical. For example *Cystopus candidus* is changed to *C. capsellæ* E. H. because the fungus is said to grow mostly on Capsella and every fungus should be named as far as possible from the host it infests. In like manner *Cystopus cubicus* becomes *C. compositarum* E. H.; *Phytophthora infestans*, *P. solani* E. H.; *Peronospora sparsa*, *P. rosæ* E. H., etc. In the same way the author puts his initials after many old genera e. g., Phytophthora and Peronospora, or substitutes other names, e. g. *Zoospora* E. H. for *Plasmopara*, because he concieves the name to have been originally employed in a different sense from that in which it is now used or in which he employs it. The other idea running through the book and occupying at least a third of it is that bacteria originate from plastids developed inside of the cells of fungi, and that we shall never make any progress in the study of animal and plant diseases due to bacteria until we determine from just what fungi they originate. The potato rot, for example, is due to bacteria developed from the broken down mycelium of the fungus *Phytophthora infestans*.

"If now one keeps for a long time in observation under the microscope such an escaped mass of plasma [from the mycelium or conidia of Phytophthora] one beholds, just as in the cases already mentioned by us, the freeing of the plastids, their change into micrococcus, and the elongation, division, etc. of these." (P. 82). In *Peronospora ficariæ* also "the origin of the microörganisms is unquestionable, but until now I have not been able to follow them further. These organisms are visible in a fresh section in the interior of the leaf tissue of the host." (P. 134.) The converse of this proposition is also true i. e. that under certain conditions bacteria change back again into the original fungus, the growth of certain yeast cells into mycelium being cited as a case in in point. "If these [bacteria] arise from definite fungi by the finally free development of the plastids, it must also come to pass that the micrococcus, which is the first product of the freed plastids, will again give rise to the higher fungous form from which it originated. Of this the first well known and precise example is the history of the development of the beer yeast." (P. 105.). The author who is a graduate of one of the German Universities, formerly held a chair of botany in one of them, and has been writing books similar to this one for the last 30 years claims to have seen the change from fungous plastids inside of mycelia or spores into genuine free swimming bacteria, rods and cocci. This change is difficult to bring about artifically, requiring long watch-

ing at the microscope and the partial exclusion of air from the preparation. Figures are given of these plastids and of the bacteria. All of which reminds us of the proof of miraculous healing by holy water at certain wells, viz., " the well is with us to this day." The author complains that nobody reads his books, but this cannot be charged against the writer who has patiently waded through the whole of this one, to very little profit, however, it must be confessed. The absurdities, however, are not so numerous as in the author's *Phytopathology*, published in 1868. Therein may be found, full fledged or in embryo, most of the queer notions here set forth and also many others.

ERWIN F. SMITH.

ZOOLOGY.

The Cruise of the Princess Alice.—The zoological material obtained by the Prince of Monaco during the past summer cruise of his yacht, the Princess Alice, is abundant and valuable. The fortunate capture of a sperm whale in the vicinity of the yacht, off the coast of Terceira Island, resulted in the acquisition of some rare specimens of the animal kingdom which otherwise might never have been known. From the Prince's narrative of the voyage we learn that the cachalot was the " catch " of some Portugese whalers with whom the Prince arranged to secure what portions of the animal he wished, especially the brain. Unfortunately some days elapsed before the skull was penetrated, and then the brain was found to be in too advanced a stage of decomposition to be of use for preservation. Meantime a large number a parasites were collected from the stomach, the digestive organs, the blubber and the skin of the animal, and the contents of the stomach secured for examination. While in the act of death the whale ejected several large cephalopods which it had only just swallowed, as was evident from their perfect preservation. These were also obtained by the Prince for his collection. Amongst them were three large specimens, each over one meter in length, of a species probably new, of the little-known genus Histioteuthis; also the bodies of two other immense cephalopods so different from all hitherto known that it is impossible to place them in any genus or even family of this order. M. Jonbain proposes for them the name *Lepidoteuthis grimaldii*. One of these specimens is a female, of which the body, or visceral sac after prolonged immersion in formol and alcohol, still measures 90 cm. in length, from which it is

estimated that the length of the complete animal would exceed 2 meters. The surface of the sac is covered with large, solid rhomboidal scales, like those of a pine cone. The fin is very powerful, forms one-half of the length of the body and is not furnished with scales.

When the stomach of the whale was opened it was found to contain over a hundred kilogrammes of partially digested debris of cephalopods, all of them of enormous size. The crown and tentacles of a Cucioteuthis were identified. This genus has hitherto been known only by few fragments. The muscular arms, though shrunken and contracted by the preserving fluid, are as thick as those of a man, were covered with great suckers, each armed with a sharp claw, as powerful as those of the larger carnivora. More than one hundred of these suckers remain adhering to the arms.

Another cephalopod found in the stomach of the whale is provided with a large fin, in the skin of which are enclosed certain photogenic organs. The form of the body suggests a new species, but as the head is wanting, it cannot be positively identified.

These cephalopods are all powerful swimmers, and very muscular. They appear to belong to the fauna of the deep intermediate waters, an almost unknown region. They never come to the surface, no do they lie on the bottom of the sea. Their great agility prevents their capture in nets, hence it would seem that the only way to obtain these interesting gigantic creatures is to kill the giant who feeds upom them and rescue the fragments from his huge maw.

Accordingly, for the next season's cruise, the Princess Alice is to have, in addition to her present fittings, those of a sperm whaler, or else to have as a compainion a special whaling tender.

The further working up of the material in hand is being pushed forward with energy, and interesting results are anticipated. (Nature, Jan., 1896.)

Australian Spiders.—Among the new Arachnida reported from New South Wales are three species of Nephila; *N. fletcherii*, *N. edwardsii* and *ventricosa*. These are described and figured by Mr. W. J. Rainbow in the Proceeds. of the Linn. Soc. N. South Wales. The author includes in his paper some interesting observations on the habits of Nephilæ and their supposed bird-snaring propensities. Representatives of this genus abound in tropical and subtropical regions. Their webs are composed of two kinds of silk; one yellow, exceedingly viscid and elastic, the other white, dry and somewhat brittle. The latter is used for the framework of the web, the guys and radii, and the former

for the concentric rings. These snares are at varying heights, sometimes within reach, again 10 to 12 feet from the ground, but always in a position exposed to the rays of the sun. The diameter is also variable, from 3 feet upwards. One seen by Gräffe in the Fiji Islands (probably a Nephila) constructs a web 30 feet in diameter.

These snares are strong enough to entrap small birds. In the author's opinion the web is not set for such game, and the spider does not feed on her ornithological victim. In the cases where she has been observed with her fangs in the body of the ensnared bird it is probable that it is for the purpose of hastening the death of the bird in order to prevent its injuring the web in its struggles to escape.

Spiders of the genus Nephila are easily tamed. Although exceedingly voracious, they can nevertheless exist for many days without either food or water. They pair in autumn. The sexes inhabit the same web for a considerable time, the female in the center and the male on the upper edge of the web. His efforts to ingratiate himself in the favor of his mate are not always successful. It not infrequently happens that he has to retire from her presence minus two or three legs. "Ultimately says the author, he succeeds in attaching himself in the requisite position, and performing the necessary act of fecundation." (Proceeds. Linn. Soc. N. South Wales, [2] Vol. X, Pt. 2, 1895.)

Autodax iëcanus.—According to Mr. Van Denburg, *Autodax iëcanus*, a black Salmander first found in Shasta Co., California, is a nocturnal forager. It usually walks slowly along, moving one foot at a time, but is capable of rapid motion when necessary. At such a time it aids the action of the legs by a sinuous motion of the whole body and tail. In addition to being prehensile, the tail is put to a third use. When caught the animal will often remain motionless, but if touched will raise the tail and strike it forcibly against the surface upon which it rests, and accompanying this action with a quick motion of the hind legs, will jump from four to six inches, rising as high as two or three inches. Mr. Van Denburg finds that the species has a wide distribution in California. (Proceeds. Calif. Acad. Sci., Vol. V, 1895.)

Reptiles and Batrachians of Mesilla Valley, New Mexico.—The following list may be worth publishing as a contribution to the more exact knowledge of the distribution of animals in New Mexico. It may be relied upon as correct, as all the species have been identified by Dr. L. Stejneger, and the specimens are to be found in the U. S. National Museum. The valley about Las Cruces, where most of the species were obtained, is 3800 ft. above sea-level, its extreme sides rise

to about 5000 ft. The records marked Lane Coll. are based on specimens obtained by Mr. Lane of Las Cruces, mainly by purchase from the Mexicans.

(1.)* *Bufo lentiginosus* v. *woodhousei.* Common about the town.

(2.) *Rana pipiens* v. *brachycephala.* Common in suitable places.

(3.) *Amblystoma tigrinum.* Not rare about the town. A large speimen found by Mr. J. Schmidt.

(4.) *Cistudo ornatus.* Common about the town.

(5.) *Sistrurus edwardsii.* Close to the College building.

(6.) *Heterodon nasicus.* Close to the College, rather common.

(7.) *Coluber emoryi.* One near Las Cruces, April, 1894 (J. M. Walker).

(8.) *Pituophis sayi.* Our commonest snake. One specimen had the head-scales arranged as in the so-called genus *Churchillia.*

(9.) *Bascanion testaceum.* One specimen.

(10.) *Thamnophis dorsalis.* Frequent, the commonest snake after *Pituophis.*

(11.) *Lampropeltis pyrrhomelas.* H. B. Lane Coll.

(12.) *Lampropeltis splendida.* Lane Coll.

(13.) *Diadophis regalis.* Lane Coll.

(14.) *Arizona elegans.* Lane Coll.

(15.) *Rhinocheilus lecontei.* Lane Coll.

(16.) *Liopeltis vernalis.* Lane Coll.

(17.) *Tantilla nigriceps.* Lane Coll.

(18.) *Leptotyphlops dulcis.* Lane Coll., also one obtained by Prof. C. H. T. Townsend.

(19.) *Eumeces obsoletus.* Not rare near the College.

(20.) *Cnemidophorus tessellatus.* Common about the mesquites bushes.

(21.) *Cnemidophorus perplexus.* Lane Coll.

(22.)* *Sceloporus magister.* One in Coll. Exp. Sta., one in Lane Coll.

(23.)* *Uta stansburiana.* Our commonest lizard, abundant on the college campus.

(24.)* *Crotaphytus wislizenii.* One. Remains of beetles in stomach.

(25.)* *Crotaphytus baileyi.* Apparently not uncommon. One had two young *Phrynosoma modestum* in its stomach.

(26.) *Phrynosoma cornutum.* Common. At Lamy and Santa Fé it is replaced by *P. hernandezii,* which in the neighborhood by Santa Fé ascends to 7475 ft.

(27.) *Phrynosoma modestum.* Common. There also occurs a bluish mutation.

The Death Valley Expedition, much further west, obtained 56 reptiles and batrachians, of which only five (those marked with an asterisk) are common to our list. It is especially noteworthy that there is not a single snake in common.

—T. D. A. COCKERELL, N. M. Agr. Exp. Sta.

Professor Cope's criticisms of my drawings of the squamosal region of Conolophus subcristatus Gray; (Amer. Natural., Febr., 1896, p. 148–149) and a few remarks about his drawings of the same object from Steindachner.—In the February Number of this Journal Prof. Cope makes the following remarks: "Dr. Baur again denies that the exoccipital [paroccipital] articulates with the quadrate in certain genera of the Iguanidæ and gives some figures of that region in the *Conolophus subcristatus*, to sustain his allegation. Unfortunately, though he seems to have taken the elements apart, as I suggested that he do, he did not put them together in their original relation when he had them drawn. I now give two drawings traced from the skull of the same species given by Dr. Steindachner. As these plates represent exactly the characters, which I have observed and described in allied genera, I regard them as correct. It will be observed that there is a considerable contact between the exoccipital and the quadrate. There is also contact with the supratemporal [prosqamosal] and probably with the paroccipital [squamosal]. *The articulation of the quadrate with the exoccipital is universal in the Iguanidæ.*"

To this I have to reply the following: 1. The single elements of the skull of Conolophus were not taken apart at all. The quadrate, prosquamosal, and squamosal of the right side were separated from the rest of the skull, in such a way, that they remained together in natural position. The corresponding left side of the skull remained intact. All this was done two years ago, without the advice of Prof. Cope. My figures were drawn with the camera-lucida and are absolutely correct in every respect. I have two other skulls of Conolophus; several of Amblyrhynchus, Iguana and Cyclura. In all I find the same condition. I have not to change a single word in my original description nor a line in my drawings. The quadrate is not supported by the par occipital [exoccipital Cope] in the Lizards, as Cope stated, but by the squamosal [paroccipital Cope], the prosquamosal [supratemporal Cope] taking also part, if present. The paroccipital does not even touch the quadrate.

Colo., May 11, 1890. Food, *Euphoria inda*, one specimen, almost whole; *Cicindela* sp., fragments; hymenopterous insects, broken, rather large, thorax black, abdomen red.

(7.) *Pyranga ludoviciana*. ♂. Shot by Mr. P. Lowe, Big Arroyo, Colo., May 14, 1890. Food, fragments of a blow-fly (*Lucilia* or *Calliphora*); fragments of a beetle (perhaps a longicorn); and some green eggs, apparently those of a *Smerinthus*. These eggs were numerous, but I found no fragments of the ♀ moth in which they must have been when eaten.

(8.) *Salpinctus obsoletus*. Shot by Mr. W. P. Lowe, Big Arroyo, Colo., May 14, 1890. Food, fragments of bettles, including a weevil.

(9.) *Geococcyx californianus*, ♂. Shot by Mr. W. P. Lowe, Big Arroyo, Pueblo Co., Colo., Dec. 5, 1889. Food, grasshoppers (*Acrididæ*), *Ophryastes tuberosus* and perhaps another allied species, and a blue-green rugose metallic fragment of an unknown insect.

(10.) *Ampelis garrulus*, ♀. Shot by Mr. W. P. Lowe, Badito, Huerfans Co., Colo. Food, berries of juniper (*Juniperus communis*).

(11.) *Aphelocoma woodhousei*, ♂. Shot by Mr. W. P. Lowe, Badito, Huerfans Co., Colo. Food, fragmentary seeds (papilionaceous?), fragments of bones of a small passerine bird.

(12.) *Merula migratoria*. Cusack Ranch, Custer Co., Colo., April, 1888. Food, seeds and geodephagous beetles.

Mr. Lowe is responsible for the identification of the birds shot by him; he sent me only the stomach-contents.

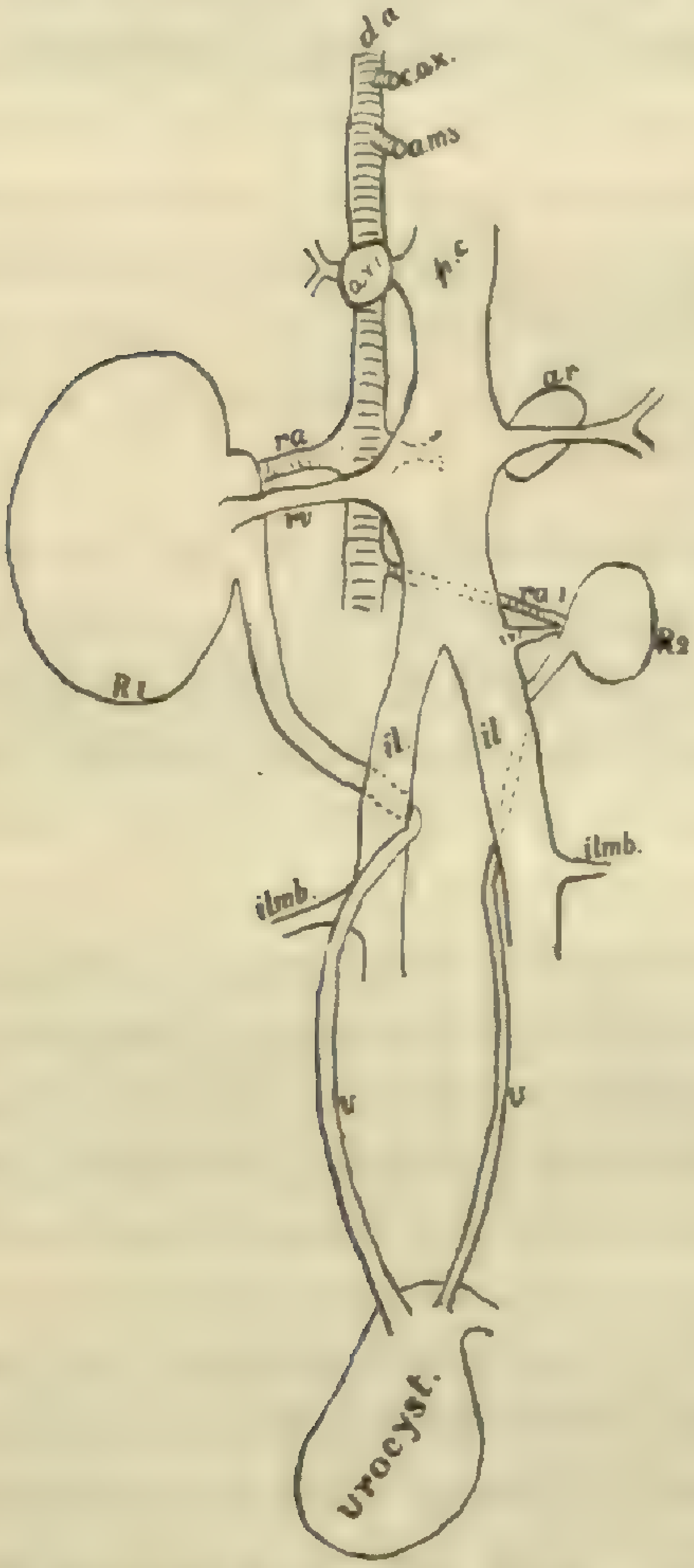
—T. D. A. Cockerell, N. M. Agr. Exp. Sta.

The Manx Cat.—A correspondant of the Zoologist notes an interesting fact concerning a Manx cat in his possession. This tailless cat took of its own accord a mate of the normal type, and from the union resulted a litter of three, which like the mother lacked tails. Friendly relations continued to exist between the parent cats until six successive litters had been produced, each litter in turn showing to a less degree the mother Manx cat's influence upon the form of the progeny, as may be seen in the following table compiled by the owner of the cats.

Litters.	Tailless.	Half tail.	Normal tail.
1	3	0	0
2	2	1	0
3	1	2	0
4	0	2	1
5	0	1	2
6	0	0	3

It would be interesting to carry the experiment further and see if a union of the Manx cat with one of her own race would result in restoring with the same regularity with which she lost it, the power to produce her own type. (Revue Scientif. T. 4, 1895.)

A case of Renal Abnormality in the Cat.—Anomalous condition of the renal organs and accompanying blood vessels was recently disclosed in a dissection in this laboratory. The accompanying diagram explains the phenomenon. The left kidney was a miniature of the right though functional. The dimensions of the right kidney in another subject of equal size as the specimen under discussion were found to be—length 3 cm., width 2 cm. and dorso-ventral thickness 15 mm.; the left os is natural being slightly smaller than this. The dimensions just given may be regarded as normal.



In the subject whose renal anatomy has been here figured, the measurements of the right kidney were as follows: length 4 cm. breadth 2½ cm. and thickness (dorso-ventral) 19 mm., considerably above the normal as one would expect when the extremely small size of the left kidney is considered. The dimensions of the latter were as follows: length 12 mm., breadth 8 mm., and thickness or dorsoventral diameter 5 mm., less than one-third the dimensions of the right kidney.

Upon hardening, staining and sectioning in the usual way the glomeruli and uriniferous tubules were found to be normal though, the presence of a small amount of fat in the kidney was noted. The histological condition of the kidney and the presence of the left ureter, which, though smaller than the right was clearly functional, proved that the left kidney was of value in the vegetative processes of the organism. The right renal artery (*ra*) was, as one would expect larger than the left (*ra*). The postcava (*pc*) in this cat was divided very far forward in the lumbar region to form the common iliac veins, causing the left

renal vein (*rvi*) to empty into the left common iliac. This variation from the normal in postcaval structure is by no means uncommon.

Letters in the figure, not referred to in the text are as follows: *da.* dorsal aorta, *cax.* cœliar axis, *a. m. s.* anterior mesenteric artery, *rv.* vein from right kidney, R_1. right kidney, R_2. left kidney, *ar.* and *ari.* left and right adrenal bodies with accompanying veins. *Il* left and right common iliacs, *ilmb.* ilio-lumbar veins *u* ureters, *urocyst* urinary bladder.—F. L. WASHBURN, Biological Laboratory, University of Oregon.

Zoological News.—Mr. O. F. Cook has published a monograph of Scytonotus. He considers this genus to be the most specialized of the Polydesmid Myriapoda, basing his conclusion on its secondary sexual characters. He recognizes nine species as belonging to the genus. (Ann. New York, Acad. Sci., VIII).

A gigantic Cephalopod, supposed to be a new species of Architeuthis, was driven inshore on the eastern side of the bay of Tokyo. A description of it, illustrated with drawings, is published by K. Mitsukuri and S. Ikeda. It is characterized by shape of its fins and of its beaks, the unequal lengths of the sessile arms, and other minor details. (Zool. Mag., Vol. VII, 1895).

Prof. Gegenbaur has in the Morphologisches Jahrbuch for the year 1895, instituted a study of the clavicle and the elements adjacent to it and the scapular arch. He calls attention to the fact that there are two elements in the position of the former in Dipnoi, Crosopterygia and Chondrostei. He then shows that the element nearest the scapula is retained in some of the Stegocephalia, while the anterior and distal element is increased in length. He calls the former the cleithrum, and retains for the latter the name clavicle. The clavicle only remains in the existing order of Batrachia, and higher groups, while the cleithrum only remains in the higher fishes, beginning with Lepidosteus and Amia.

According to Dr. Delisle the cranial capacity of the Orang-Outang averages 408 cubic centimeters. (L'Anthropologie Tome, VI, 1895.)

Ranke's researches show that the weight of the human brain is much greater in proportion to the weight of the spinal cord than in any other vertebrate. (Correspondenzblatte).

Dr. E. Rosenberg publishes in the Morphologisches Jahrbuch for 1895, an investigation into the reduction of the number of the incisor teeth which is seen in the human species. He shows: first, that the loss of

the external incisor, which was first pointed out by Cope, and which has been observed independently by several others, is frequently observed in Europe as well as in America; second, that the loss of the first inferior incisor is also not very uncommon in Europe and that the final reduction of the inferior incisors, should it take place, will be by the loss of this tooth and not by that of the external incisor as in the superior series. He, therefore, believes that the ultimate formula of the incisive dentition in man will be $I\frac{1}{1}$, and not $I\frac{1}{2}$, as Cope left it.

ENTOMOLOGY.[1]

The Segmental Sclerites of Spirobolus.—The structure of the segments of Diplopoda has long been a morphological puzzle. On account of the possession of two pairs of legs they have in a general way been supposed to be double segments, that is, formed by the coalescence of two distinct embryonic or theoretical segments. Toward a morphological demonstration of this idea there has been little progress. Indeed, there are many facts which give grounds of suspicion as to its correctness. Among these may be noticed that the double footed state does not occur in the embryo at all, and that the segments which in the adult bear two pairs of legs either do not exist in the newly hatched larva or do not bear any legs at that stage, the newly hatched diplopod larva having but three pairs of legs, the posterior of which is attached to the fourth segment (at least in the Polydesmoidea). Moreover, all Diplopoda have apodous segments not differing otherwise from those which bear legs; also all Diplopoda have segments which bear but one pair of legs, and yet have not been found to be greatly different from the others. Growing Diplopoda acquire segments by intercalation in front of the last. The segment is added at one moult, the legs for it at the next. As the possession of two pairs of legs has been the occasion of the theories of duplex segments, these facts are the more relevant as objections, since more difficulties are introduced than are disposed of by the theories.

The existence of pluræ in the Oniscomorpha has long been known, and for a less period in the Colobognatha and Limacomorpha. In the other orders these elements of the segmental ring are so thoroughly coalesced or eliminated that their existence was theoretical until their

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

discovory in Stemmatoiulus. In that form, however, the multiplicity of peculiar characters weakens the application of homologies to the other orders, unless these can be based on structural facts. It is thus a matter of interest that the existence of pleuræ in another Diplopod order can be affirmed.

Some weeks since in examining large African Spirobolidæ I noticed what seemed to be traces of pleural sutures. On mentioning this fact to Mr. F. C. Straub who was studying with me, he called my attention to a specimen of *Spirobolus marginatus* Say which proved to be very remarkable. Possibly it was collected just after moulting, before the sclerites had become coalesced, or it may have been merely an individual anomaly. At any rate, it had on each side an obliquely longitudinal white line across each segment above the pedigerous lamina, indicating a pleural element about as broad as the lamina. That this is the pleural suture seems very probable, on theoretical grounds and more so that on the surface a special striæ followed the line of white.

What is more remarkable, this line was met above by two others which were transverse, dividing the segment into three subequal parts. These two lines extended completely over the animal, the space between them being somewhat greater above. There is also a median longitudinal suture, and a lateral just below the pore, thus dividing the dorsal portion of the ring into twelve subequal parts. The posterior of the transverse sutures follows the depression found in the segments of Spiroboli and usually called "the suture" in descriptions. The anterior line and the median line are indicated by minute differences in the sculpture, which would not have been noticed had not the white line drawn attention to them. It should be added that the lighter color was not due to anything inside or outside the segmental wall, but was in the wall itself and clearly indicated some structural difference. The phenomenon was exhibited by the anterior and middle segments of the body, becoming indistinct caudad. In all cases the pattern was the same; the whole series of lines could be made out on many segments, and there were no other similar lines or discolorations. The lines were not straight if examined under a microscope, even the median showing minute irregularities. Median sutures are known in four or five of the Diplopod orders and hence may reasonably be expected in all.

Had only the median line been marked as related, there would have been no hesitation in supposing that a median suture was indicated. Theoretical considerations only stand in the way of the reasonable presumption that the other exactly similar lines indicate sutures. If such an interpretation is allowed we are brought to the position that

the segmental ring of Spirobolus consists of sixteen sclerites; twelve dorsal, two pleural and two ventral or pedigerous laminæ. It will be seen that only the last tend to indicate a transverse division of the segment, and in no Diplopod as yet has there been shown a transverse suture carried around the segment and dividing it into two parts. Only the legs and the parts necessarily connected with them, such as the pedigerous laminæ and nerve ganglia are duplicated. Even in the Oniscomorpha, Limacomorpha and Colobognatha where the pleuræ are most distinct, there is not the slightest indication that they were ever divided, and as they are the elements to which the pedigerous laminæ are next related, their evidence is more important than any drawn from the dorsal parts of the segment.

There is another way, however, in which a diplopodous animal might be developed from a monopodous ancestor. Alternate segments may have been suppressed, while the corresponding legs have been preserved. For such a supposition we have the analogy of the Chilopoda, where the pedigerous segments alternate with more or less rudimentary segments. In this case, however, the legs have been lost, that is, we must suppose so if we claim the analogy. Such a theory, while no more fantastic than the other, is probably no nearer the truth. After theoretical explanations have been exhausted we may, perhaps, learn that the double-footed condition is a peculiarity of this group of animals, not explainable by any general morphological considerations, but *sui generis,* after the manner of the branched segmental appendages of the Crustacea.—O. F. Cook.

Secretion of Potassium Hydroxide.—Mr. O. H. Lalter has some further notes[2] on the secretion of potassium hydroxide by *Dicranura vinula* and similar phenomena in other Lepidoptera. He finds that the imagines of eight species secrete from the mouth an alkaline fluid on emerging from the pupa. The three species of *Dicranura* wear what is called a shield, derived from the pupa case as they emerge, and they subsequently remove it by their legs. He finds that the strength of the solution in *D. vinula* is about 1.4 grm. of potassium hydroxide in every 100 ccm. of liquid. The mesenteron of the same species develops an anterior dorsal diverticulum for storage of the alkali during pupal life.—*Journal Royal Mic. Society.*

Lake Superior Coleoptera.—Mr. H. F. Wickham publishes[3] an admirable list of Coleoptera from the southern shore of Lake Supe-

[2] Trans Ent. Soc. Lond., 1895, 399-312.

[3] Proc. Davenport Acad. Nat. Science, VII, 125–169.

rior. More than 200 species are enumerated in this list which have not before been credited to the region of the Lake. The collections were made at Bayfield, Wisconsin, during June and July.

The following introductory remarks are of sufficient general interest to be quoted at some length. The time for an accurate map of the faunal regions of the continent has not yet come—nor will it before another century at least of careful investigation has enabled us to fix approximately the range of the rarer forms of insect life. It is evident to any one who will read with care and with some understanding of the general principles of distribution, that many of the recent theories as to the division of our country into "life-zones" have very little foundation in fact. If better proof were wanting of this, we might point to that of authors changing from year to year their arbitrary arrangement of our zoö-geographical regions—uniting to-day two or three of those of older authors, and separating them again a few months later on. All this may or may not be progress, but it will all have to be gone over again in the light of a wider knowledge than seems to be at present in the possession of certain writers who cannot rest without having first shown us that all previously conceived ideas are totally wrong, and that their explanation of the distribution of life is the only plausible one. A single group of animals may or may not indicate in a general way the lines of distribution followed by a larger number—but it is manifestly unreasonable to hope for a stable method of division of a country into life-zones before the life of that country is well-known.

EMBRYOLOGY.[1]

The Effect of Lithiumchloride upon the Development of the Frog and Toad egg (R. fusca and Bufo vulgaris.)[2]—The results of the series of experiments performed in the histological laboratory at Münich with this salt seem of no little interest, and especially is this the case with the result obtained with a 0·5 per cent solution. In every instance the eggs were placed in the solutions (varying from 1 per cent to 0·2 per cent) between a half and an hour and a half after fertilization.

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts reviews and preliminary notes may be sent.

[2] A. Gurwitsch, cand. med. Anat. Anz., XI, 65–70.

The blastula obtained with the 5% solution the author attempts, with some degree of plausibility, to make out to be of far reaching morphological importance. Whereas in all other cases development was either more or less hindered or was abnormal, in this case it was entirely symmetrical. The first indication of gastrulation appeared as a ring sinking about the equatorial plane and embracing the entire circumference. Sections showed a large mass of what the author calls passive yolk cells or endoderm forming the lower half, while the upper half, composed of a layer of ectoderm and one of active endoderm, forms a sort of cap covering it.

At a later stage this cap almost includes the lower passive entoderm, and, as the author points out, forms a gastrula that, if the passive yolk be removed, very closely resembles the gastrula of *Amphioxus.* From this it may seem more or less probable that the primitive amphibian gastrula may have been radially symmetrical, and that bilateral symmetry appeared later. Further it appears that the upper or large invagination of the amphibian egg is not the blastopore, but this is represented by the entire circle including the yolk plug.

It may be noted also, that if instead of supposing the passive entoderm to be removed, it be supposed to be greatly increased, one then has a gastrula of the meroblastic type.

Another point of interest is the manner in which the cells of the so-called "active endoderm," or those bordering the equatorial ring, proliferate. This proliferation according to the author has already begun when the invagination of the outer surface commences ; so that instead of there being a *pushing in* of the outer surface, as the process is usually described, there seems to be a *pulling in.* Whether this process is due to the "cytotropism" described by Roux for the cells of the dividing frog egg, or to the taking up of the space occupied by the absorbed contents of the blastula cavity, as described by Hatschek for *Amphioxus*, is not clear.

The embryos obtained differ from those obtained by O. Hertwig with NaCl, in that the brain capsule does not close up and the dying away of the brain matter does not take place, and again instead of the animal cells breaking down as in NaCl, it is the yolk cells that crumble away.

Finally one abnormal lithiumchloride embryo has an adverse significance for the concrescence theory.

It is to be hoped that the author intends later to publish a more extensive paper, which shall be more fully illustrated.—F. C. K.

ANTHROPOLOGY.[1]

An Inquiry into the Origin of Games.—An examination of the games of the Far East (Korea, China and Japan) and a comparison of them with certain games of the North American Indians as explained by Mr. F. H. Cushing, has induced Mr. Culin (Korean Games, with Notes on the Corresponding Games of China and Japan, by Stewart Culin, Director of the Museum of Archeology and Paleontology of the University of Pennsylvania, Philadelphia, 1895) to believe that the true game in the American and Asiatic region referred to, is a traceable descendant of primitive religious divinatory formulæ, reaching back to a time in the process of human development, when man freshly inspired by the phenomena of earth an sky, symbolized in his ceremonies the directions of the four winds, and foretold fate or fortune with arrows.

Because American Indians divine by arrows, because archery, and sets of arrows corresponding in number to Asiatic cosmic divisions, arrow derived grave posts, and guild tallies notched and named like arrows, still survive in Korea, and because arrow like rods are still used there in divinatory formulæ by fortune tellers, Mr. Culin has been led to regard arrow divination as a primitive and original form of fortune telling, and while the totemic arrow marks on short round gambling

Fig. 1. Haida Indian gambling stick suggesting derivation from the arrow. One of a set of 32 bearing devices of the totemic animals of the worlds' quarters supposed to have been derived (traceably perhaps through an intermediate set marked with colored ribbons) from arrow shaftments such as were used by the McCloud River Indians.

sticks of northwest coast Indans are urged as indications of the arrow ancestry of the latter, the same interesting suggestion is made as to the cylindrical earthen stamps from Ecquador and the round and flat engraved cylinders from Babylonia. Twenty-three out of the ninety-seven Korean games described (though in many cases the clue is not

[1] This department is edited by Henry C. Mercer, University of Penna, Phila.

stated) and particularly games played on diagrams like the Korean *Nyout*, (*Pachesi*) or chess, are held to suggest a primitive divining board

Fig. 2. Cylindrical earthenware stamp from Ecuador suggesting derivation from the arrow. It bears a highly conventionalized device representing a bird. It's striking resemblance to the Haida gambling sticks suggests its own derivation from the carved shaftments of arrows and furnishes also a clue to the probable origin of the Babylonian seal cylinders.

—the world, with the quarters of the four winds "the heavens above and the earth beneath," where the relations of arrows thrown, scattered red or distributed, symbolized the early callings of man upon fate, the first soothsayer's translation of unseen causes into the events of life.

The investigation deals with evanescent and elusive conditions and of necessity the family tree of games is often vague and disjointed. An etymology, arrow notches on a card, scorings on sticks, the pas-

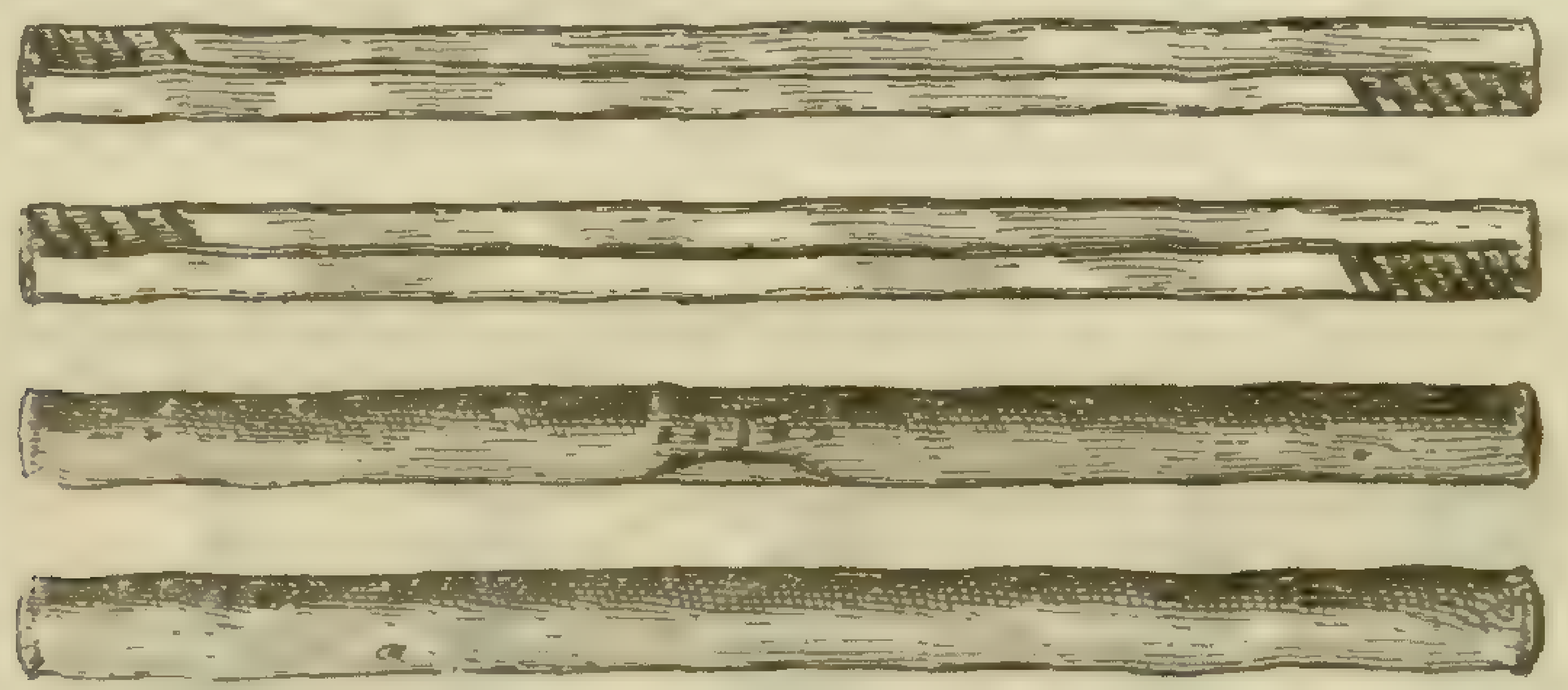


Fig. 3. Count Staves of wood used by Kiowa Indians suggesting derivation from arrows, emplosed in the game of *Zohn ahl*, they are inscribed with marks resembling arrow decoration and shaftment.

times of Indians in America and of far Orientals in Asia depending often upon the impartial testimony of the investigator have seemed to lead hnmanity backward in the cases and countries cited, not to the flight of birds, the observed instincts of animals, or the virtues of plants or minerals, but to the arrow as the ancestral symbol of the human necromancer. The Korean game of *Nyout*, where the throwing of marked sticks scores on a dotted diagram, seems related to divi-

nation because its arrangement of dots looks like magical diagrams in an ancient Chinese book of divination while there are throws, and

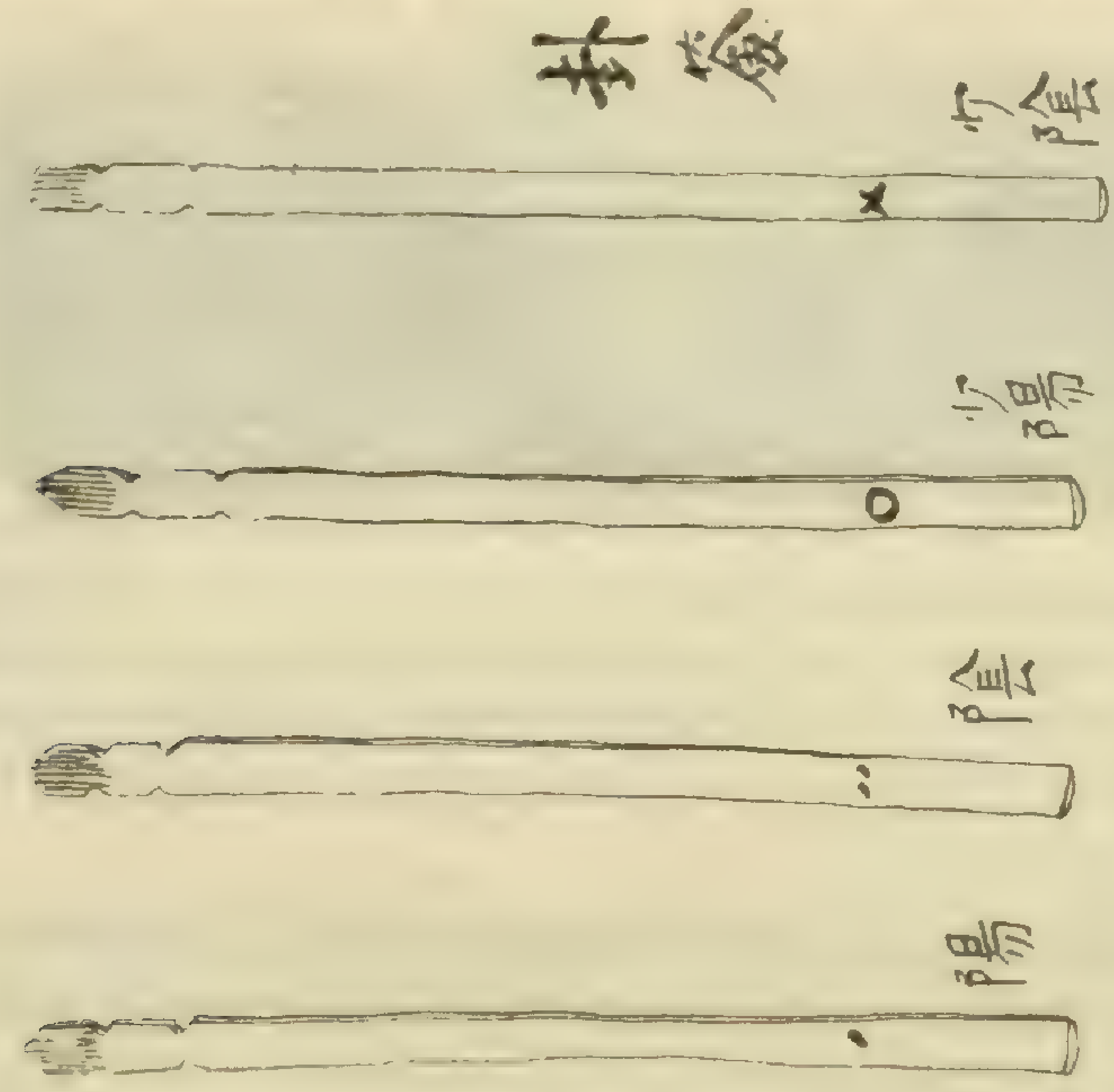


Fig. 4. *Kwa Zsin* Chinese (wooden) divining splints. Four of a set of sixth-four, suggesting by their notched points and name (*tsim* resembling *tsin*, arrow) a derivation from arrows.

arrangements, and suites, and figures in the game that seem to connect it with chess, and with dice and backgammon and other Korean dice and board games, thus, we are told, putting the latter familiar and Europeanized class of games into the line of succession from the primitive formulæ of the arrow diviner. Long narrow Korean playing cards, resembling a set of Chinese lottery arrows similarly marked,

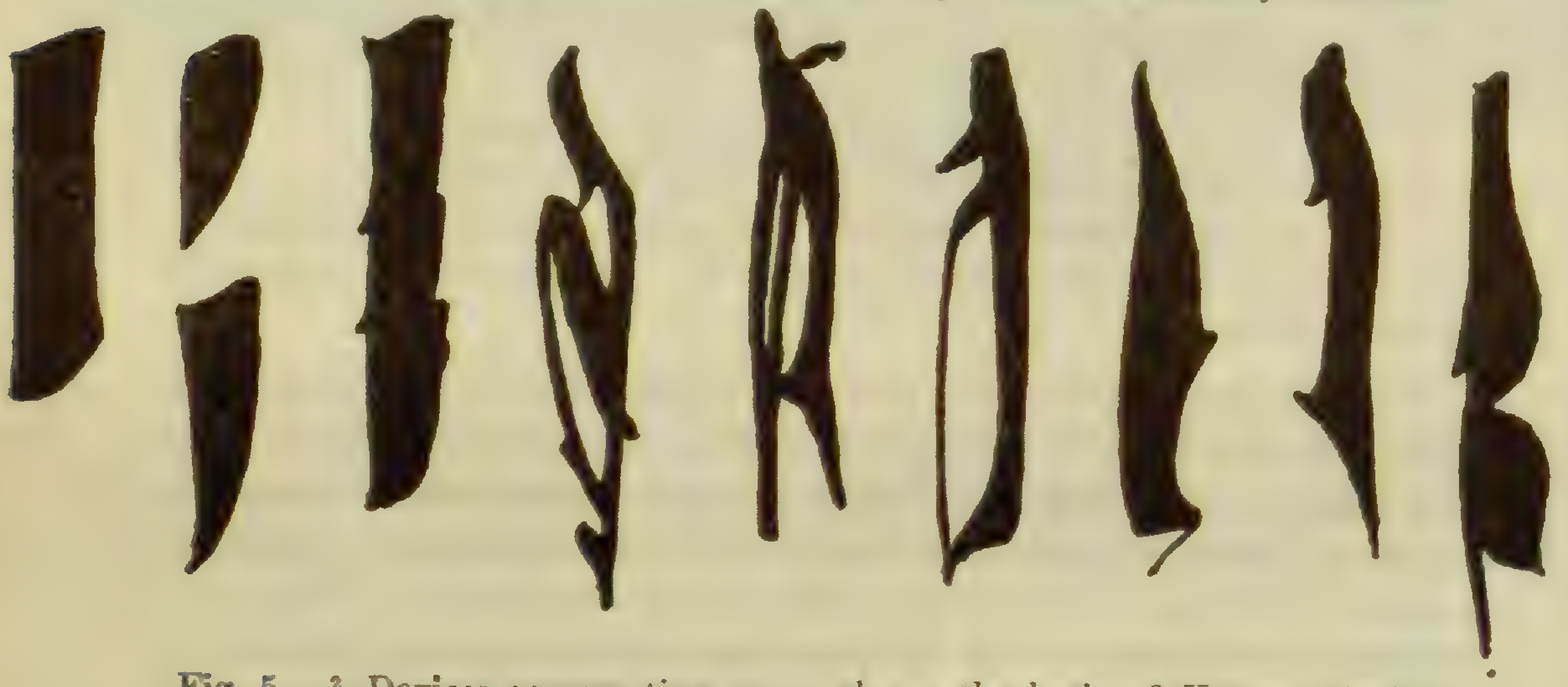


Fig. 5. $\frac{2}{3}$ Devices representing numerals on the backs of Korean playing Cards, (*htou-tjyen* fighting tablets). The devices suggests in their shape a derivation from the cut cock feathers on arrows.

and with arrow feathers painted on their backs refer us strikingly to the arrow, and this fact, illustrated by aseries of surprising pictures is one of the telling features of Mr. Culin's book. Whether we agree or not, whether we prefer to wait till more evidence is in for regions like parts of Australia, Tasmania and the Andaman Islands where man appears never to have had arrows, and whether we believe that we have reason to doubt that the notion of the four world quarters ever was universally impressed upon humanity the original suggestions of Mr. Culin pointing out new and seemingly widespread relations among games and tracing or seeking to trace in them fresh illustration for the story of human development, is of importance and interest.

Following further the author's dignified and always sympathetic presentation of the subject into a description of other games which sometimes, like the counting out rhymes of children, are regarded as less conscious survivals of the diviners' doings, sometimes as mere festive or athletic pastimes, we gather pleasing evidence of the world kinship of children in the record (often illustrated by native Korean artists in color), of blind man's buff, leap frog, horse stick, tug of war, stone fighting, pop guns, tops, tilt ups, and jack stones. Too briefly the pages reflecting remembered joys of youth tell of the loosened waters of a brook breaking, if they can, a juvenile dam, of hostile kites sawing their abraisive strings as they soar, of violet whipping, of shovel playing, of youthful mouths crammed with cherries, to be eaten without swallowing the stones, and of dragon flies, caught in spider webbed hoops by children reciting poems and released with unconscious cruelty when impaled with paper banners. But new aspects of an ever present floral sympathy in the land of cherry blossoms and the chrysanthemum are revealed to us when we learn of such Japanese names for bands of combatants as "spring willow blossom," "summer rest forest," and "autumn garden," shouted across the green turf in the foot ball game.

Notwithstanding the similarities urged between some of the arrow games of North America and their Asiatic representatives we look in vain in the book for suggestion of contact of races, or proof of migration. Lines of investigation such as other observers might choose in tracing the rolling of stone discs scored in motion with sticks or arrows (*Chungke*) from the Sandwich Islands to Georgia, the author eschews as unfruitful and inconclusive "unless supported by linguistic evidence." His valuable and original investigation has not essayed to furnish new light as to the geographical origin of the human race but has rather multiplied the evidence showing that man's mind has worked alike everywhere.—H. C. MERCER.

PSYCHOLOGY.

Prof. Mark Baldwin on Preformation and Epigenesis. —In the last number of the NATURALIST was republished from *Science*, Prof. Baldwin's observations on my presentation of the contrasted hypotheses of the development of mind.[1] One of these theories was supposed to be in accordance with the evolutionary doctrine of preformation, the other was thought to bear the same relation to that of epigenesis. Prof. Baldwin asks why the three theses arranged under epigenesis may not with equal or greater propriety be arranged in the preformation column. He believes that consciousness has had an influence in directing the course of evolution in accordance with the "general law now recognized by Psychologists under the name of Dynamogenesis—*i. e.*, that the thought of a movement tends to discharge motor energy into the channels as near as may be to those necessary for that movement." He also says, "I do not suppose that any naturalist would hold to an injection of energy in any form into the natural processes by consciousness. The psychologists are, as Mr. Cattell remarks, about done with a view like that." Prof. Baldwin also remarks that "Prof. Cope can say whether such a construction is true in his case." He adds that "it is only the physical basis of memory in the brain that has a causal relation to the other organic processes of the animal."

To reply to the last question first. The facts seem to show that conscious states do have "a causal relation to the other organic processes of the animal." I have gone into this subject briefly, but more fully than can be done here, in Chap. X of my book on the "Primary Factors of Organic Evolution" (1896). The evolution of the brain, the organ of consciousness, would indicate this, as well as the evidence for Kinetogenesis or evolution by motion. This would follow, if the doctrine of Dynamogenesis referred to by Prof. Baldwin be true, at the psychic end of the process, and if acquired characters be inherited, as required by the doctrine of epigenesis If then consciousness has such a function, the question arises as to its immediate mode of action. Prof. Baldwin says "only the physical basis of memory has a causal relation," etc. This proposition I can accept, and it is true whether that physical basis be due to a conscious state called a sense-impression, or not. But the directions of the acts (motions) which flow from that physical basis are very various in organic beings, having adaptations

[1] See Primary Factors of Organic Evolution, 1896, p. 14.

to as many ends as there are benefits to be obtained. It is evident that the physical basis of memory undergoes a change from the condition in which it is first produced. Its component parts are evidently rearranged in accordance with some purely psychic factors, *i. e.*, in accordance with qualities and properties which are only appreciable by conscious states. One may suppose that a reflection of the physical basis of a memory may be transmitted to different parts of the cortex, and that in one part it is located in accordance with one criterion of classification, and in another region in accordance with another criterion. In other words, the representative functions of the brain control the structure of the physical basis of memory, or cause a modified reproduction of it. These representative functions may be of the simplest —*i. e.*, they may consist only of criteria of size, color, utility, etc., or they may be more complex, involving judgments, concepts, etc. Finally, no criteria can violate the ultimate "forms of thought," which are essentials of all representative mental action. These, in short, are the fundamental reasons why mental conditions may be believed to direct the course of energy, without increasing the amount of that energy.

The relation of this factor of evolution to the the theories of Preformation and Epigenesis may be now considered. The reason why I believe that the process of mental evolution has been and is at bottom epigenetic, is because there is no way short of supernatural revelation by which mental education can be accomplished other than by contact with the environment through sense-impressions, and by transmission of the results to subsequent generations. The opinion is simply a consistent application to brain tissue of a doctrine supposed to be true of the other organic structures. The injection of consciousness into the process does not alter the case, but adds a factor which necessitates the progressive character of evolution.

I do not perceive how promiscuous variation and natural selection alone can result in progressive psychic evolution, more than in structural evolution, since the former is conditioned by the latter. The objections to this mode of accounting for progressive structural evolution are well known, and are enumerated in my book on page 474. It is true, no doubt, that as we rise in the scale of mental faculty the capacity for acquisition increases. How far these acquisitions are inheritable is a question of detail, but no one denies, so far as I am aware, excepting consistent preformationists, that they are more or less inheritable. It is to be supposed that the longer special aptitudes are cultivated the more likely they are to be inherited, precisely as the ef-

fects of constant use of an organism are inherited, while sports and mutilations are not inherited. The importance of the social influences among men on which Prof. Baldwin justly lays so much stress, consists in the fact that they are continuous in their operation, and produce permanent habits. This accounts for the phenomena referred to by him when he remarks that "the level of culture in a community seems to be about as fixed a thing as moral qualities are capable of being; much more so than the level of individual endowment. This latter seems to be capricious or variable, while the former moves by a regular movement and with a massive front." Here we have portrayed exactly what occurs in structural evolution. The habitual influence of the environment, internal and external, conditions the steady advance, while sports produce only temporary effects or are effective only in proportion to their ratio to the entire movement.

In an essay published in Science of March 20th, 1896, Prof. Baldwin comments on the lectures of Prof. Lloyd Morgan, in support of his own doctrine of Social Heredity. This is the name he has applied to this transmission of habits through their persistence in societies, so that the young acquire them through imitation or instruction, without the intervention of physical heredity. As a foundation for this view he disputes the necessity of any inheritance of acquired habits by the inheritance of the nervous mechanism which they express, and denies therefore that use is a necessary agent in the evolution of such habits. In order to prove that instincts are not "lapsed intelligence" he says; "The intelligence can never by any possibility create a new movement or effect a new combination of movements, if the apparatus of brain, nerve and muscles has not been made ready for the combination which is effected. This point is no longer in dispute," etc. Immediately before this, however, he says. "But let us ask how the intelligence brings about coördinations of muscular movement. The physchologist is obliged to reply; "Only by a process of selection (through pleasure, pain, experience, association, etc.,) from certain alternative complex movements, which are already possible for the limb or member used."

It is granted in the last quotation that pleasure, pain and other conscious states, select the motions which become habits. Such selection is intelligent, and such act is an expression of intelligence, though of the simplest sort. All that Prof. Baldwin alleges is that intelligence is impotent to construct the mechanism of new habits out of mechanisms already too far specialized in definite directions to permit such a reorganization of structure. This truth in nowise contradicts

the construction of the mechanism of new habits from tissues capable of reconstruction or of modification, a quality which resides very probably in brain tissue, or at least certainly has resided in it at various stages of organic evolution, when new "selections through pleasure, pain, experience, association, etc.," were made; otherwise the selection would have been impossible. This is the history of all the other tissues, and why not of brain tissue? Though Prof. Baldwin denies the necessity of the Lamarckian Factor, he admits it in this doctrine of selection; and his denial of inheritance, only covers the case of physchological sports, as above pointed out. Hence he both admits and denies both Lamarckian and Weismannism.

Weismannism has recently struck the physchological camp, and in Prof. Baldwin and in Mr. Benjamin Kidd, we see some of its recent effects. But since the biologists have generally repudiated Weismannism, the evolutionary physchologists must try and get along without it. Nevertheless, as above remarked, Prof. Baldwin's "Social Heredity" is a real factor, especially in human evolution; but as it is not heredity, I think it should have a new name, which shall. be less confusing.

E. D. Cope.

Psychologic Data Wanted.—For purpose of extended comparison I wish data as to habit, instinct or intelligence in animals, above all, minor and trifling ones not in the books, *useless* or *detrimental* ones, and the particular *breed*, *species* or *genus* showing each. Purring, licking, washing face, kneading objects with fore-paws, humping back, and "worrying" captured prey (like the cat), baying (at moon or otherwise); urination and defecation habits (eating, covering up, etc.); disposition of feces and shells in nest; rolling on carrion; cackling (or other disturbance) after laying; eating "afterbirth" or young; sexual habits; transporting eggs or young; nest-sharing; hunting partnerships or similar intelligent associations; hereditary transmission of peculiarities; rearing young of other species with resulting modification of instinct; feigning death; suicide; "fascination;" are examples. Circular of information will be sent and full credit given for data used, or sender's name will be confidential, as preferred.

Answer as fully as possible, always stating age, sex, place, date (or season), species, breed, and whether personally observed.

R. R. Gurley, M. D.

Clark University, Worcester, Mass.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

Nova Scotian Institute of Science..—March 9th.—The following paper was read: "Some Illustrations of Dynamical Geology in Southwestern Nova Scotia," by L. W. Bailey, Esq., M. A., Ph. D.

HARRY PIERS, *Secretary.*

Boston Society of Natural History.—February 19th—The following papers were read: Mr. Outram Bangs: "The Terrapin an Inhabitant of Massachusetts." Dr. Joseph Lincoln Goodale: "The Vocal Sounds of Animals and the Mechanism of their Production."

March 4th.—The following paper was read: Prof. F. W. Putnam: "Symbolism in Ancient America.".—SAMUEL HENSHAW, *Secretary.*

New York Academy of Sciences—Biological Section.—February 7th, 1896.—Dr. J. G. Curtis in the Chair.

A communication from the Council was received asking that the Section take action on Rep. Hurley's bill "To fix the standard of Weights and Measures by the adoption of the metric system of weights and measures."

On motion of Dr. Dean, the Section approved the bill and the Secretary was directed to express the entire commendation of it to the Council.

Dr. Arnold Graf read a paper on "The Structure of the Nephridia in Clepsine." He finds, in the cells of the intra-cellular duct, fine cytoplasmic anastamosing threads which form a contractile mechanism. These are stimulated by granules which are most numerous near the lumen of the cell, and thus a peristalsis is set up which moves the urine out of the duct. In the upper part of the intra-cellular duct, the two or three cells next to the vesicle or funnel have no distinct lumen, but are vacuolated; the vacuoles of the first cell being small, those of the second larger, and so on, till the vacuoles become permanent as a lumen. He explains the action of the first cell as being similar to the ingestion of particles by the infusorians. The matter taken up thus from the funnel by the first cell is carried by the rest, and so on till the cells having a lumen are reached. The presence of the excretum causes the granules to stimulate the muscular fibres of the cells; penstalis results and the substance is carried outwards. The character of this contractile reticulum offers an explanation of the structure of a cilium as being the continuation of a contractile reticular thread.

N. R. Harrington, in "Observations on the Lime Gland of the Earthworm," described the minute structure of these glands in *L. terrestris,* and showed that the lime is taken up from the blood by wandering connective tissue cells which form club-shaped projections on the lamellae of the gland, and which pass off when filled with lime. The new cell comes up from the base of the older cell and repeats the process. This explanation is in harmony with the fact that in all other invertebrates lime is laid down by connective tissue cells. Histological structure and the developmental history confirm it.

Dr. Bashford Dean offered some observations on "Instinct in some of the Lower Vertebrates." The young of *Amia calva*, the dogfish of the Western States, attach themselves, when newly hatched, to the water plants at the bottom of the nest which the male Amia has built. They remain thus attached until the yolk sac is absorbed. As soon as they are fitted to get food they flock together in a dense cluster, following the male. When hatched in an aquarium they go through the same processes. The young fry take food particles only when the particles are in motion, never when they are still. The larvæ of Necturus also take food particles that are in motion.—C. L. BRISTOL, *Secretary.*

American Philosophical Society.—January 17th.—Prof. Hilprecht presented a paper on "Old Babylonian Inscriptions, Chiefly from Nippur," Pt. ii.

February 21st.—Prof, A. W. Goodspeed read a paper on the Röntgen method, with demonstration. Remarks were made by Prof. Houston, J. F. Sachse, Prof. Robb of Trinity College, and Prof. Trowbridge of Cambridge.

March 6th.—The following paper was presented: "Eucalypti in Algeria and Tunisia from an Hygienic and Climatological Point of View," by Dr. Edward Pepper.

Academy of Natural Sciences of Philadelphia—Anthropological Section.—February 14th.—The following papers were read: Dr. Allen on "Prenasal Fossæ of the Skull;" Dr. Brinton on "Human Hybridism;" Dr. McClellan, Skulls and Photographs exhibited.

CHAS. MORRIS, *Recorder.*

The Academy of Science of St. Louis.—February 17, 1896.—Dr. Adolf Alt spoke of the anatomy of the eye, and, by aid of the projecting microscope exhibited a series of axial sections representing the general structure of the eye in thirty-one species of animals, comprising two crustaceans, the squid, three fish, two batrachians, two reptiles, ten birds, and eleven mammals.

Professor F. E. Nipher gave an account of the Geissler and Crookes tubes and the radiant phenomena exhibited by each when used in connection with a high-tension electrical current of rapid alternation, and detailed the recent discoveries of Professor Röntgen, showing that certain of the rays so generated are capable of affecting the sensitized photographic plate through objects opaque to luminous rays. Attention was also called to the experiments of Herz and Lodge with discharges of very high tension alternating currents, which showed that by the latter certain invisible rays are produced, which, like the Röntgen rays, are capable of passing through opaque bodies, such as pitch, but differing in their refrangibility by such media.

March 2d.—Mr. F. W. Duenckel presented a comparison of the records of the United States Meteorological Observatory, located on the Government building in the city, with the record for the Forest Park station, showing that the daily minimum averaged decidedly lower at the Forest Park station than in the city, while the wind averaged decidedly higher for the city station.

Professor E. E. Engler spoke on the summation of certain series of numbers.—William Trelease, *Recording Secretary.*

SCIENTIFIC NEWS.

The *Journal of Comparative Neurology*, which is now entering upon its sixth volume, has its editorial facilities considerably enlarged by the addition to the staff of Dr. Oliver S. Strong, of Columbia College. Professor C. L. Herrick continues as Editor in-Chief. The Managing Editor for 1896, is C. Judson Herrick, to whom business communications should be addressed at Denison University, Granville, O. Editorial communications may be sent to either of the three editors.

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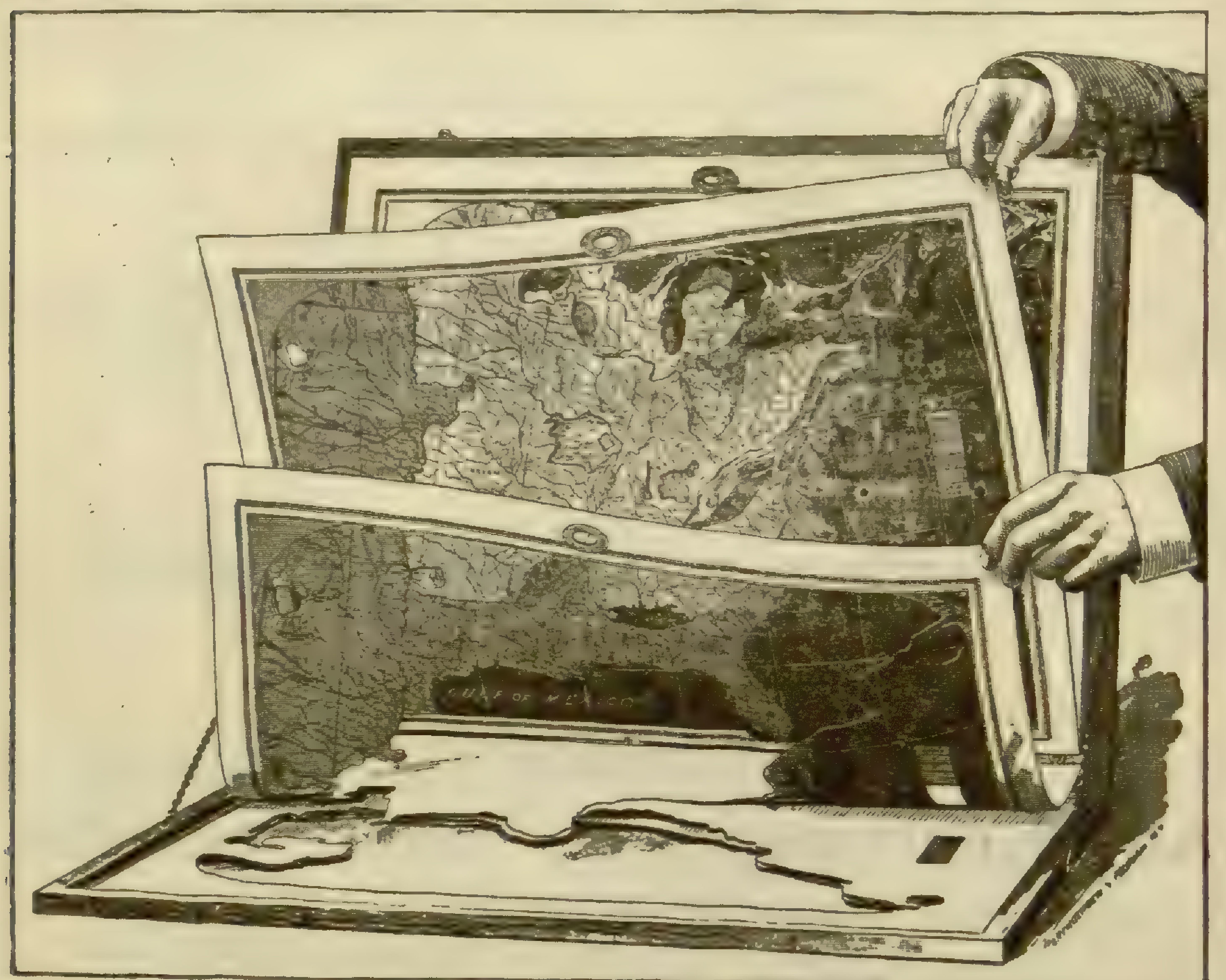

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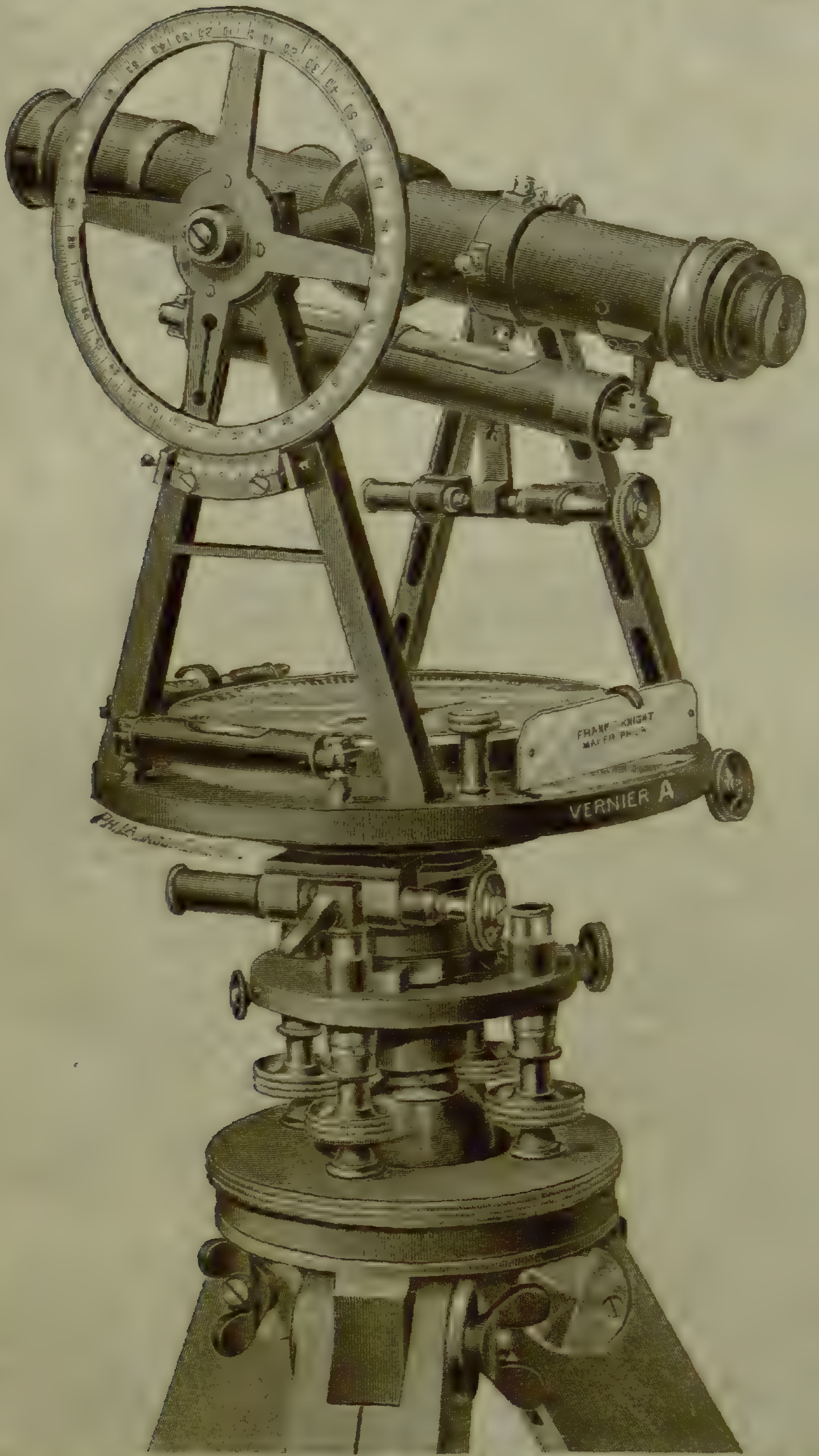

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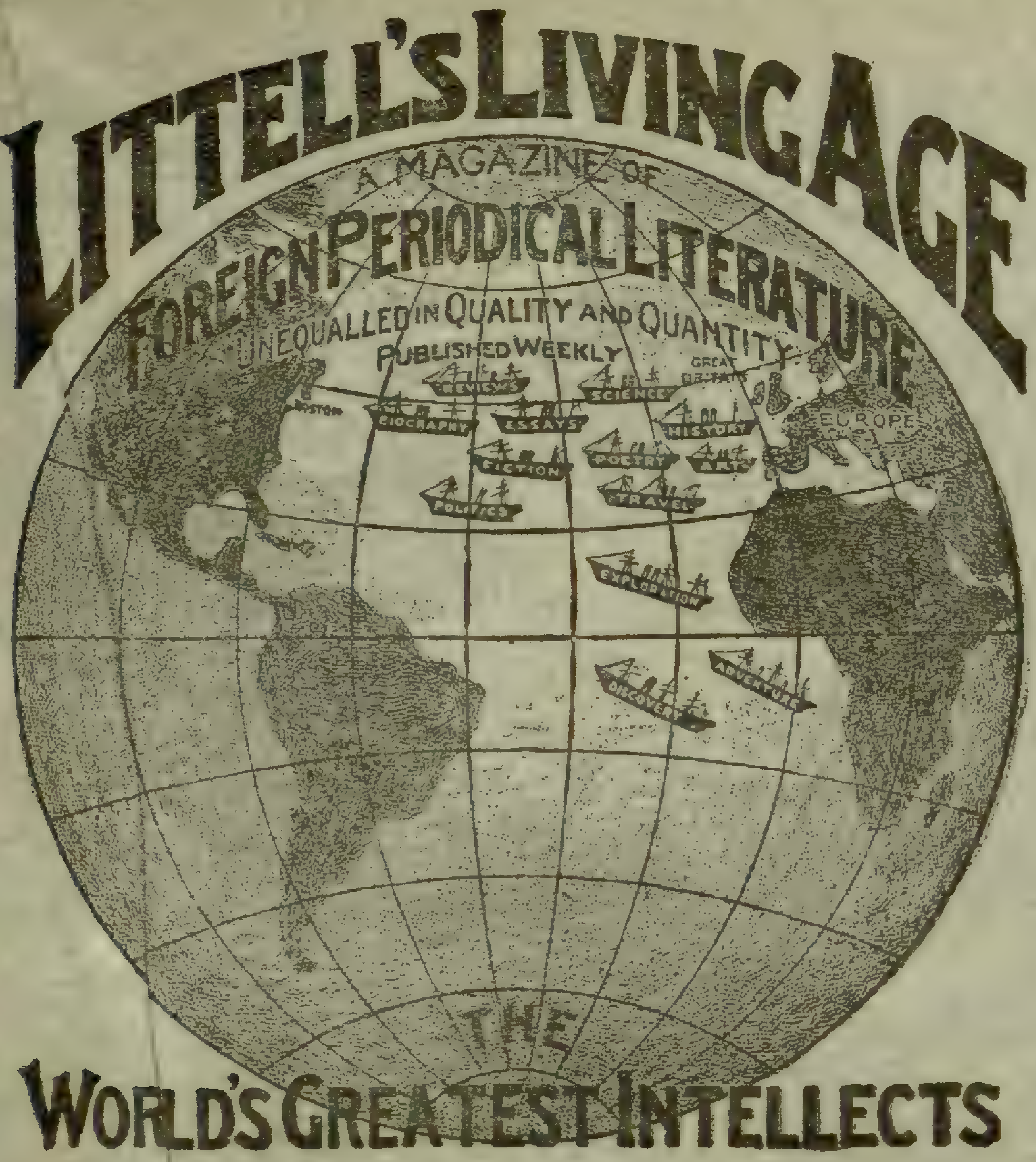

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THE PROBABLE INFLUENCE OF DISTURBED NUTRITION ON THE EVOLUTION OF THE VEGETATIVE PHASE OF THE SPOROPHYTE.

By Geo. F. Atkinson.[1]

In this paper the discussion of the influence of nutrition, applies chiefly to that source of nutrition in plant organs provided with chlorophyll, and presupposes, in general, that the ordinary physiological processes, other than the one which is termed carbon assimilation, are normal. In all such plants some development of this vegetative part of the plant must take place before spore production, or fruiting, of a kind which represents a real increase at the time, can be accomplished. Some apparent, but not real, exceptions to this might be noted. In germination of the spores of Oedogonium, frequently spore production takes place without the development of any such vegetative part of the plant, but there is no real increase of the plant substance. This kind of spore production is only a means, perhaps, to tide over some condition unfavorable for the elaboration of the vegetative phase of the plant, which is present at the time and place. In Coleochæte, germination of the oospore results in the formation of a cellular mass, which

[1] Cornell University.

is larger than the oospore, breaks the enclosing wall, and the cells escape as a number of zoospores in place of one. But in this oospore are the stored products of carbon assimilation of the parent chlorophyll phase of the plant, and this case only differs from that of the Bryophyta, in that the sporophyte becomes separated, with stored products, from the gametophyte, before the differentiation of the spores.

In the higher plants many cases of bulbs, corms, tubers, etc., might be cited to show that the development of the sporophylls, and even fruit, might take place without the accompaniment of chylorophyll bearing organs. But here also the bulbs, corms, etc., represent, in the stored products of carbon assimilation, the preceeding green leaves. In certain ferns, as Osmunda cinnamomea, the sporophyll, which is completely differentiated from the vegetative leaf, appears first in the spring, and could mature its spores without the aid of the vegetative leaves of that season, but the green leaves of the previous season formed the necessary carbohydrates, which are stored in the rhizome and rudimentary leaves during the winter and in fact the sporophylls and sporangia are partly developed at the close of the previous season.

We might say, then, that in general, all spore production in plants, which themselves assimilate carbon dioxide, is necessarily preceeded by a greater or lesser development of chlorophyll bearing organs. This may appear to be a too well known axiom for even the brief discussion here given, but it is necessary in view of what is to come to have this axiom well in mind. Chlorophyll bearing organs, or tissues, then, as compared with sporogenous organs or tissues, are, in point of time within the life cyle, *primary*, while the latter are *secondary*. This proposition should not be regarded as opposed to the primary evolution of the sporophylls as compared with the foliar organs of the sporophyte. It applies only to a comparatively limited extent of time; to the usual cycle between the vegetative and fruiting phases; to the ontogenetic, not to the phylogenetic, development. It applies with equal force to plants in which either the gametophyte or the sporophyte forms the chlorophyll bearing organ.

There is a strong tendency in nature to an economy in the distribution of the food supply between foliar, and sporophyllary, organs; between the vegetative and fruit products of the plant. This is well seen in the varying sizes of plants having varying amounts of food supply, where with limited food supply small plants have few and small leaves accompanied by a limited out-put of fruit (other conditions are considered normal); while with increasing amounts of food, other things being equal, each of these plant products is increased, though not in the same ratio. With high feeding the vegetative increase shows a higher ratio than the fruiting. The food may be so abnormally abundant as to cause an abnormally abundant vegetative growth, accompanied in some cases with rudimentary fruit, or in others with the entire suppression of the fruit. In some rare cases it may be accompanied by the transformation of the sporophyllary organs to vegetative ones.

These facts teach that the fruit product, or sporophyllary development of plants is very sensitive to food supply, requiring a certain amount of food for perfection in even small quantities, increasing with additional food supply up to a given point, when it decreases again to zero; or in rare cases the sporophyllary organ may be transformed to a vegetative one, so antagonistic has the ratio between the vegetative and sporophyllary organs become because of the abnormally favorable conditions for vegetative growth. In addition to the sensitiveness which the fruit organs exhibit to varying amounts of food derived from the soil, they are very sensitive to disturbances in the supply of carbohydrates as a result of carbon assimilation in the vegetative organs, especially of a kind which partly or completely cuts off that supply. This is well seen in the diminished crops as a result of injury to the leaves at critical periods from insects or fungi, or as result of unfavorable meteorological conditions.

When the nutritive supply of the carbohydrates is suddenly disturbed by certain kinds and amounts of injury to the foliage leaves, or by pruning severely, thus cutting off a large number of forming or developed leaves, certain parts of the plant, either simple or in a rudimentary condition of development,

have the function of carbon assimilation forced upon them to save the plant from destruction and to provide for the development of the fruiting organs. As is well known, latent buds, which have been in a dormant condition, may be, for years, are frequently in such cases stimulated to development and form leafy shoots. It is a very common occurrence as a result of severe pruning or of injury to the ordinary leaves for young flower axes, or buds, to develop leafy shoots with other flower buds in the new leaf axils. There is a tendency here to force the vegetative function upon the young flower axes, and if this influence is felt before the specialized character of the floral organs has been developed from the cells at the apex of the axis, the development of these organs will be deferred, while these cells assume the form and function of vegetative organs. This is a matter, perhaps, of common observation as a result of severe pruning, and in some plants can be very easily demonstrated by trial. But it serves well to show the influence of disturbed nutrition on other or dormant parts of the plant when the function of the existing vegetative leaves is arrested. That function is forced from the ordinary and well developed organs upon undeveloped or rudimentary ones, which readily under this influence adapt themselves to continue this important office. This must not be regarded as an attempt to explain the development of adventitious or supernumerary buds, etc., or latent buds, in all cases, into leaf branches. Local stimuli, and a number of other causes at times, call forth leafy shoots from these. Nevertheless, in view of what has been said above, the following proposition might be formulated. Nutrition disturbed, and the development of the fruit product of the plant being threatened, by the loss of carbon-assimilating organs, the function of the latter may be taken up by some other part of the plant, either rudimentary or undifferentiated, their development into said organs being a direct result of that disturbance.

In the cases dealt with above, the function is either transferred to latent vegetative organs, or to undifferentiated tissues. It is, therefore, simply and easily comprehended. Observations have been made, however, which tend to show that in

the Angiosperms the vegetative function can be assumed not only by the floral envelopes, but also by the sporophyllary organs, more commonly by the macrosporophylls, or gynæcium. In some cases these open and expand into green leaves with the ovules, in a more or less imperfect stage of development, exposed. Perhaps no definite experiments have been carried out to demonstrate the cause of this transformation of form and function. It is supposed to be due to either excessive nutrition, or to some injury to the vegetative system of the plant. While such cases are unsatisfactory because of the lack of definite tests, they indicate that the sporophylls can assume the form and function of foliar organs when there is a disturbance of nutrition of a kind considered above. Since these sporophylls are, from a morphological standpoint, considered homologous with the green leaves, this change of function is not incompatible with that theory.

In the Pteridophytes direct experimentation proves beyond a doubt, that the sporophylls can be made to assume the form and function of the foliar organs by cutting off the latter, thus disturbing the nutrition and forcing the vegetation function on the sporophylls. The experiments performed upon *Onoclea sensibilis* and *O. struthiopteris* may be cited. In these cases after cutting off the early developed vegetative leaves the sporophylls appeared later in all stages of transformation, some complete vegetative leaves with only vestigial remnants in the form of rudimentary indusia to indicate to which series of organs they primarily belonged in the ontogeny of the plant. Between these and perfect sporophylls all gradations of intermediate forms occurred, the terminal portions of the sporophyll and of the pinnæ always being more fully expanded, while the basal portions of the same partook more or less completely of the true sporophyll. The details of the experiment and of the gradations of the development are given elsewhere, and cannot be dwelt upon here.

As an outgrowth of these experiments and observations a second proposition may be formulated as follows. Disturbed nutrition, resulting from the loss of the carbon assimilating organs of the sporophyte (vegetative leaves), may, and does,

force the vegetative function on the sporophylls, causing them to develop into more or less complete vegetative leaves.

The experiments on Onoclea convince me that there are a number of Pteridophytes, as well as Phanerogams, which would yield the same results following the amputation of their leaves, when carefully conducted, especially in those plants where, during one season, the vegetative leaves are developed sometime in advance of the sporophylls. Plants like some of the Lycopods would make extremely interesting ones to work with, and especially in the case of some of these should I expect to see a transformation of the sporophylls into vegetative leaves. This would be entirely in harmony with the relation and development of these organs. In species of Lycopodium and Selaginella all gradations between sporophylls with normal sporangia and the vegetative leaves can be found. The transitional stages are marked by the gradual degeneration of the sporangia on some of the leaves, the sporophyllary character being shown only by vestiges of the sporangia. Bower has shown how the strobilus of the Lycopods elongating by apical growth would result in the increase in the number of the sporophylls, and that the demand thus made on the vegetative system for nutrition would result in the transformation of some of the sporophylls to foliage leaves, accompanied by a corresponding sterilization of some of the sporangia. Practically it would disturb the balance of nutrition between the sporophyllary and vegetative systems, the effect being the same as it would be if some of the foliage leaves were destroyed.

In view of the ultimate purpose of this paper the question must be raised here as to whether this transformation of the sporophylls to foliar organs is a case of reversion, or whether it is an advance of a primary organ to a secondary organ of the sporophyte. It is my conviction that the latter alternative is the logical and true one, that we can by experimentation demonstrate phylogeny in ontogeny. Bower[2] has called attention to the importance which must be attached to the fact that the primary function of the sporophyte was not only the production of spores, but an increasing number; that the increase in

[2] Ann. Bot., VIII, pp. 345–365, 1894.

the mass of sporongenous tissue was necessarily accompanied by a sterilization of potential portions of the mass for purposes of protection, support, and for the conduction of nutritive material. From this condition he reviews the theoretical grounds for the relegation of the spore-producing cells to a superficial position, and the eruption of outgrowths on which the sporangia are supported, citing as illustrations of the early conditions of these outgrowths the strobilus of Equisetum and Phylloglossum. From the latter he traces the development of the elongated and branched leafy stem of species of Lycopodium by continued apical growth of its strobilus, while the sporangia on some of the lower sporophylls would be arrested, and the sporophylls themselves would develop as foliage leaves. For these and similar reasons elaborated by himself, he concludes, rightly I think, that the sporophylls are, from a phylogenetic point of view, *primary*, while the foliage leaves are *secondary*.

All the evidence which we have points to the fact that in the early development of the sporophyte, it was entirely dependent upon the gametophyte for nutrition including the supply of carbohydrates. The expanded green prothalloid structure performed the same function for the sporophyte, that foliage leaves of the sporophyte do in plants where this becomes independent of the gametophyte. This is practically true now in all the thalloid liverworts, and in all the Bryophyta is the sporophyte practically dependent upon the gametophyte for this function. In most of the Pteridophytes the sporophyte is dependent upon the gametophyte for its carbohydrates during the embryo stage. In some of the Pteridophytes, in the Gymnosperms, and in the Angiosperms, the gametophyte has entirely lost the function of carbon assimilation, this function being solely performed by parts of the sporophyte.

What influences led to the gradual transfer of this function of the gametophyte to parts of the sporophyte? Nutritive disturbances have been shown to play a very important part in the formation of sporophyllary organs quantitatively, in varying ratios between the vegetative and sporophyllary structures with increased food supply; in a tendency to produce a natural

but variable equilibrium between these two functional kinds of organs; and especially in the transformation of sporophyllary organs to vegetative ones. If these disturbances, especially in the nature of partial or complete loss of carbon assimilating organs of the sporophyte produce such an effect, why should there not be a similar influence brought to bear on the sporophyte, when the same function resides solely in the gametophyte, and a disturbing element of this kind is introduced? To me there are convincing grounds for believing that this influence was a very potent, though not the only one in the early evolution of sporophytic assimilatory organs. By this I do not mean that in the Bryophyta, for example, injury to the gametophyte would now produce distinct vegetative organs on the sporophyte, which would tend to make it independent of the gametophyte. But that in the Bryophyte-like ancestors of the Pteridophytes an influence of this kind did actually take place appears to me reasonable.

In the gradual passage from an aquatic life, for which the gametophyte was better suited, to a terrestrial existence for for which it was unadapted, a disturbance of this function was introduced. This would not only assist in the sterilization of some of the sporogenous tissue, which was taking place, but there would also be a tendency to force this function upon some of the sterilized portions of the sporophyte, and to expand them into organs better adapted to this office. As eruptions in the mass of sporogenous tissue took place and sporophylls were evolved, this would be accompanied by the transferrence of the assimilatory function of the gametophyte to some of these sporophylls. Even the protophylls may have originated by the eruption of certain of the sterile portions of the sporophyte under the influence of disturbed nutrition.

The sporophyte from its nature presented greater possibilities in the way of the elaboration of a complex, robust, perennial inhabitant of terrestrial zones. Increased sporogenous tissue was necessarily accompanied with a more bulky structure, which then necessitated a differentiation of its tissue by sterilization of certain external, then internal, parts for protection and circulation. Robust types of land plants could more naturally be developed from such a phase than from the ex-

panded and delicate gametophyte. When the sporophyte had largely assumed this function of the gametophyte, and by the development of absorbing organs in the soil was enabled to live an independent existence, it became gradually established, as conditions changed, in situations where the gametophyte could not exist. It has thus become the dominating vegetative feature of most land areas, while the gametophyte in these higher forms, has become an organ entirely dependent upon the sporophyte for nourishment, or has been developed into an organ to serve a secondary purpose in the nourishment of the sporophytic embryo.

PROGRESS IN AMERICAN ORNITHOLOGY. 1886–1895.

By R. W. Shufeldt, M. D.

What I have to say here in reference to the progress in American ornithology for the past nine or ten years is prompted by the recent appearance of the second edition of *The A. O. U. Check-List of North American Birds.* Most naturalists are familiar with the first edition of this work, it having been published in 1886. It was officially promulgated by the American Ornithologists' Union, and zoologists the world over have carefully considered " The Code of Nomenclature " that formed a part of the volume. Moreover, it contained a List of North American Birds which had been prepared according to the aforesaid Code of Rules, and *classified* in accordance with the views of the majority of the committee appointed by the Union to prepare it. In so far as the orders and families of this classification were concerned, the arrangement could be appreciated at a glance by reference to the Table of Contents of the book, and, as for the List itself, it not only was intended to represent the nomenclature of the Birds, but " a classification as well " (p. 15). At the close of the volume was presented a " Hypothetical List " to which had been referred those species and subspecies the zoological status of which could not be satisfactorily determined ; and following this was a list of the fossil species of North American birds.

As the years passed by a second edition of this book was

eagerly looked for by zoologists at large, but it did not make its appearance until towards the close of December, 1895. It comes to us in the same form as its predecessor, but it does not appear to be as substantially bound or printed upon as good paper. Apart from the substitution of one member of the committee for another, it is likewise gotten out under precisely similar auspices, plans, objects and general arrangement. From it, however, has been omitted the "Code of Nomenclature," but in it are included all the new existing and fossil birds known to the committee, and which were not in the first edition of the Check-List. For this and minor changes it has but 372 pages against 392 of the original volume. In its preface it contains "extracts from the Introduction to the Code of Nomenclature," intended to serve "to explain the scope and plan of the Check-List, including the method of incorporating additions."

The second edition, then, of this work may be taken as setting forth the progress in North American ornithology as understood by a committee appointed by the American Ornithologists' Union, and for a period extending between the years 1886 and 1895 inclusive. In considering this from such a standpoint, let us first take into account the number of species and subspecies added to, or subtracted from, the List of 1886, in connection with other changes, and the same for the "hypothetical list" and for the "fossil birds." After this I will consider what improvements, if any, have been made in the matter of classification.

Designating the two volumes simply by the years of their publication, as 1886 and 1895 respectively, we find that in the first group of birds presented, or the Order Pygopodes, there were included, in 1886, *33 species* and *4 subspecies*, while in 1865 but *32 species* are given and *4 subspecies*, the change being due to the omission of *Synthliboramphus wumizusume* (Temminck's Murrelet, No. 22).

In 1886, the Great Auk (*Plautus impennis*, No. 33) was "Believed to be now extinct," while in 1895 it is confidently asserted to be "Now extinct." This being the case, we would like to inquire what place has it in a list of the *existing* birds of this or any other country? It is simply absurd to include birds that have *no existence* in nature in a list of living forms.

Passing to the second group, or Order Longipennes, we find upon comparison that in 1886 it contained *44 species* and *4 subspecies*, while in 1895 it is seen to contain *46 species* and *4 subspecies.* The additions here are the two new species *Larus barrovianus* and *Larus minutus* (a straggler). Another change in this group is the calling Pallas's Gull (*Larus cachinnans*, No. 52) the Vega Gull (*Larus vegæ* [1895]).

In the third group, or the Order Tubinares, were included, in 1886, *31 species* and *3 subspecies*, to which list was added a new species in 1895 (*Oceanodroma macrodactyla*), making *32 species* and *3 subspecies* for that year. Peale's Petrel (*Æstrelata gularis*) (No. [99]), was likewise changed to the Scaled Petrel (*Æ. scalaris*) in this group.

In 1886, the fourth group, or the Order Steganopodes, was made to contain *17 species* and *5 subspecies*, and, in 1895, *19 species* and *5 subspecies*, the increase being due to the addition of the two new species of Gannets, *Sula gossi* and *S. brewsteri.*

Coming to the fifth group, or the Order Anseres, there were contained in it in the 1886 List, *51 species* and *6 subspecies*, and, in the 1895 List, *51 species* and *8 subspecies*, the change being effected as follows: *Anas fulvigula maculosa*, the Mottled Duck, was added as a new subspecies, and *Somateria mollissima* was made the subspecies *S. m. borealis*; finally, *Chen cærulescens* was included in the list. *Camptolaimus labradorius* now being " extinct," it has no place in the List and ought not to appear there.

Group six, the Order Odontoglossæ, remains the same, each List having the *1 species* of Flamingo (*P. ruber*).

In the seventh group, or the Order Herodiones, there were to be found *19 species* and *2 subspecies*, to which were added in the 1895 List a new species and a new subspecies (*Ardetta neoxena* and *Ardea virescens frazari*). *Botaurus exilis* becomes in the new List *Ardetta exilis*, and *Ardea rufa* becomes *A. rufescens*, while the " subgenus " *Nyctherodius* is changed to *Nyctanassa.*

The Order Paludicolæ (eighth group) in the 1886 List, contained *17 species* and *3 subspecies*, to be changed in the 1895 List to *21 species* with only *1 subspecies.* This was effected by considering the subspecies *Rallus longirostris crepitans* (1886) to be the species *Rallus crepitans*, and adding also to the 1896

List the species *Rallus scottii* and *Rallus longirostris caribæus.* The subspecies *Porzana jamaicensis coturniculus* (1886) became the species *P. coturniculus* (1895). From these changes a less important one is to be noted, *viz.: Rallus longirostris saturatus* became, in 1895, *R. crepitans saturatus* (No. 211 *a*).

Passing to the Order LIMICOLÆ (ninth group), it is to be noted that in the List of 1886 there were included *66 species* and *4 subspecies,* and in 1895 these became *68 species* and *6 subspecies,* the changes being the addition of *Tringa damacensis* (a straggler); the two subspecies *Totanus solitarius cinnamomeus* and *Symphemia semipalmata inornata,* and the new species *Hæmatopus frazari.* Other changes in this group are the subgenus *Rhyacophilus* (1886) to read the subgenus *Heladromus,* and the name of the Mexican Jacana, instead of being *Jacana gymnostoma* (Wagl.), is now *J. spinosa* (Linn.).

Coming next to the Order GALLINÆ (tenth group), it is to be seen that in the 1886 List *22 species* are given and *18 subspecies,* while in 1895 there are *21 species* and *22 subspecies.* This reduction in the number of species was caused by the dropping out of *Colinus graysoni,* while the subspecies were increased by adding to the List *Oreortyx pictus confinis, Tympanuchus americanus attwateri,* and the two Turkeys, *M. g. osceola* and *M. g. ellioti. Callipepla gambeli* of the old work was corrected to read *C. gambelii.*[1]

In the eleventh group, or the Order COLUMBÆ, there were included in the List of 1886 *12 species* and no subspecies. In the 1895 List we find but *11 species,* while *4 subspecies* have been added. *Columba fasciata viosca* was recognized, while *Engyptila albifrons* (1885) became *Leptotila fulviventris brachyptera.* There was also added the subspecies *Columbigallina passerina pallescens* and the species *Columbigallina passerina* has became the subspecies *C. p. terrestris.*

There appeared *53 species* and *29 subspecies* in the twelfth group or Order RAPOTRES in 1886, while in 1895 these were increased to *54 species* and *37 subspecies.* In this group the

[1] In making these comparisons it is to be understood that they are direct between the Check-List of 1886 and that of 1895, and that the seven supplements (1889-94) and the Abridged Edition of 1889 are not taken into consideration. The second edition (1895) is taken to be the final finding of the Committee.

changes to be noted are first the addition of the subspecies *Buteo borealis harlani* (337 *d*) and the omission of *Buteo harlani* (338, 1886). *Buteo albicaudatus* becomes the subspecies *B. a. sennetti;* the subgenus TACHYTRIORCHIS being introduced between Nos. 340 and 341 in the genus *Buteo. Falco regulus* is added to the list (a straggler in Greenland). *Falco sparverius deserticolus* and *F. s. peninsularis,* two new subspecies of Sparrow Hawks, are also added, and *Falco sparverioides* is changed to *F. dominicensis. Falco tinnunculus* is also added to the 1895 List as a straggler. *Megascops asio mccallii* is now determined to be *M. a. trichopsis;* while *M. a. trichopsis* of the 1886 List now becomes *M. a. cineraceus* of 1895. Again the generic name *Ulula* is set aside for that of *Scotiaptex* of Swainson. There are also added *Megascops a. aikeni, M. a. macfarlanei, M. a. idahoensis* and *Glaucidium g. californicum* as new subspecies, and also the new species *Glaucidium hoskinsii.* The genus of Elf Owls formerly in the genus *Micrathene* have had that name replaced by *Micropallas.* The Order PSITTACI (13th group,) remains identical in the two Lists, having but the *1 species,* the Carolina Paraquet.

Following these we have the fourteenth group or COCCYZES, an Order containing the Cuckoos, Trogons and Kingfishers. All told, in 1886, there were *9 species* of these, and, in 1895, *9 species* and *4 subspecies.* These latter consist of 3 Cuckoos (*Coccyzus minor maynardi, C. americanus occidentalis* and *Cuculus canorus telephonus*), also the Texas Kingfisher (*Ceryle a. septentrionalis*). This latter was formerly *Ceryle cabanisi.* In the 1895 List *Ceryle torquata* is added [390. 1].

Next we come to the Order PICI (15th group), in which there were *23 species* and *11 subspecies* in 1886, which, in the 1895 List, stand as *22 species* and *14 subspecies.* Upon comparing the records we find that *Dryobates villosus hyloscopus* has been added as a subspecies, and also *Dryobates pubescens oreœcus. Dryobates scalaris* becomes *D. s. bairdi,* while *Dryobates stricklandi* is replaced by *D. arizonæ.*

That "highly polymorphous Order," the MACROCHIRES (16th group), containing the Goatsuckers, Swifts, "etc.," presented in the 1886 List, *26 species* and *3 subspecies.* In the present vol-

ume (1895) it is seen to include *26 species* and *7 subspecies.* The following alterations, subtractions, and additions have been made in the interim. *Antrostomus vociferus arizonæ* becomes *A. v. macromystax.* The genus *Phalænoptilus* has two new subspecies, *P. n. nitidus* and *P. n. californicus.* *Chordeiles v. minor* becomes *C. v. chapmani*, and *Chordeiles texensis* becomes *C. acutipennis texensis.* The genus of Swifts formerly in *Micropus* are now in *Aëronautes.* We have but one species of it in this country—the White-throated Swift, which, known formerly as *Micropus melanoleucus*, now is written *Aëronautes melanoleucus.* Among the Hummingbirds we have the new species *Trochilus violajugulum*, *Trochilus costæ* is changed to *Calypte costæ*, and *T. anna* to *Calypte anna*, in other words, the subgenus CALYPTE has been raised to the rank of a genus. So likewise the subgenus SELASPHORUS has been similarly dealt with, and another species added to it, *viz.: Selasphorus floresii.* Also the subgenera STELLULA and CALOTHORAX become genera, each containing a single species. *Trochilus heloisa* has been omitted from the list, and *Basilinna leucotis* added to it.

Finally, we come to the last, or seventeenth group, that vast assemblage known as the PASSERES. It will not be as convenient to deal with these as the foregoing sixteen groups were dealt with, as many of the families contain more birds than several of the other "Orders" combined, so I shall resort to tabulating the comparisons, comparing family with family.

This comparison goes to show that in 1886 there were recorded 313 species of North American *Passeres*, and, in 1895, 321, giving a gain of 8 species for the nine years, while, for the same years and interval of time, there were 117 subspecies, and, in 1895, 185, showing a gain of 68 subspecies.

With respect to the *Cotingidæ*, the single species indicated in the above Table is Xantus's Becard (*Platypsaris albiventris*). Among the *Tyrannidæ* the following changes were made: (1) The new subspecies *Myiarchus cinerascens nuttingi* was added, and also (2) the subspecies *Contopus richardsonii peninsulæ* ; (3) the species *Empidonax cineritius* is added, and (4) *Empidonax acadicus* becomes *E. virescens*, as does (5) *E. pusillus* become *E. trailli*, and (6) *E. trailli alnorum* (1895) has taken the place of

TABLE COMPARING THE PASSERES.

Families.	1886.		1895.		Remarks.
	Sp.	Subsp.	Sp.	Subsp.	
CLAMATORES.					
Cotingidæ			1		This family not in the 1886 list.
Tyrannidæ	29	7	32	9	
OSCINES.					
Alaudidæ	2	7	2	10	
Corvidæ	15	9	17	13	
Sturnidæ	1	0	1	0	(*Sturnus vulgaris*).
Icteridæ	19	7	19	8	
Fringillidæ	87	44	87	71	
Tanagridæ	5	1	6	1	(*Piranga rubiceps*).
Hirundinidæ	7	0	10	1	
Ampelidæ	3	0	3	0	
Laniidæ	2	1	2	2	(*L. l. gambeli*).
Vireonidæ	11	5	11	9	
Cœrebidæ	1	0	1	0	
Mniotiltidæ	57	9	57	13	
Motacillidæ	6	1	6	1	No changes.
Cinclidæ	1	0	1	0	No changes.
Troglodytidæ	24	7	24	16	
Certhiidæ	0	2	0	4	
Paridæ	19	9	20	15	
Sylviidæ	7	1	7	2	*Polioptila cærula obscura* added.
Turdidæ	15	7	14	10	
Total	313	117	321	185	

E. pusillus trailli; (7) *E. obscurus* (1886) becomes *E. wrightii*, and, finally, (8) *Empidonax griseus* appears as a new species.

In the family *Alaudidæ*, three new subspecies of "Horned Larks" are added to the List (*O. a. adusta*, *merrilli* and *pallida*).

In the family *Corvidæ* we have *Cyanocitta stelleri annectens* added as a subspecies, and *Aphelocoma cyanotis* as a species. In this genus occur also *A. californica hypoleuca*, *A. c. obscura* and *A. insularis.* A new subspecies of Raven is also recognized (*C. c. principalis*). Finally, and very properly, the generic name *Picicorvus* is replaced in the 1895 List by *Nucifraga* of Brisson.

Among the *Icteridæ* I note that *Dolichonyx o. albinucha* (1886) has been omitted, and that the genus *Callothrus* of Cassin has been adopted and made to contain *C. robustus*, which was formerly *Molothrus æneus* (1886). The subspecies *Agelaius phœniceus sonoriensis* and *A. p. bryanti* have been added.

The largest of all the passerine groups of birds is the family *Fringillidæ*. The following synopsis will show the changes that have been made in it since 1886:

SPECIES ADDED.

Junco ridgwayi.
Junco townsendi.
Melospiza insignis.
Euetheia canora.

SPECIES OMITTED.[1]

Carpodacus frontalis.
Zonotrichia intermedia.
Zonotrichia gambeli.
Sporophila morelleti.

SUBSPECIES ADDED.

Coccothranstes vespertinus[2] *montanus.*
Carpodacus mexicanus frontalis.
Carpodacus mexicanus ruberrimus.
Spinus tristis pallidus.
Plectrophenax nivalis townsendi.
Poocætes gramineus affinis.
Ammodramus henslowii occidentalis.
Ammodramus caudacutus subvirgatus.
Ammodramus maritimus peninsulæ.
Ammodramus maritimus sennetti.
Zonotrichia leucophrys intermedia.
Zonotrichia leucophrys gambeli.
Spizella pusilla arenacea.
Junco hyemalis shufeldti.
Junco hyemalis thurberi.
Junco hyemalis pinosus.
Junco hyemalis carolinensis.
Amphispiza belli cinerea.
Melospiza fasciata rivularis.
Melospiza fasciata graminea.
Melospiza fasciata clementæ.
Pipilo fuseus senicula.
Cardinalis c. canicaudus.
Pyrrhuloxia sinuata beckhami.
Pyrrhuloxia sinuata peninsulæ.
Guiraca cærulea eurhyncha.
Passerina versicolor pulchra.
Sporophila mocelleti sharpei.

SUBSPECIES OMITTED.

Carpodacus frontalis rhodocolpus.

[1] So long as the geographical range of a species is extended it makes not an iota's difference how that extension has been accomplished, whether it has been through human agency ("introduction"), or by other means, for when the bird becomes thoroughly established in sufficient numbers, and breeds, it is entitled to a place in any List presenting the ornis of the country into which it has come.

The Starling (*Sturnus vulgaris*) essentially gained a place and recognition, in the A. O. U. "List" from the fact that it has been successfully "introduced" from abroad. If this be granted, the Committee were guilty of very unscientific practice when they omitted the English Sparrow (*Passer domesticus*) from the "List," (also *Passer montanus*), and it can only stand as an example of how far men will allow their prejudices to carry them, and blind their scientific instincts.

[2] Spelled "*vespertina*" in 1886 edition.

Between the species *Carpodacus cassini* and the subspecies *Carpodacus mexicanus frontalis*, the subgenus BARRICA is introduced.

Progne subis hesperia has been added to the Swallows (*Hirundinidæ*), as well as *Progne cryptoleuca*, and *Petrochelidon fulva* as a straggler. The Bahaman Swallow (*Callichelidon cyaneoviridis*) having accidentally occurred on the Dry Tortugas, it introduces both the species and genus to which it belongs.

To the *Vireonidæ* were added *V. s. alticola* and *V. s. lucasanus*, as well as *V. n. maynardi* and *V. huttoni obscurus*.

In the case of the family Cœrebidæ, the genus Certhiola is superseded by Cœreba, and consequently *Certhiola bahamensis* becomes *Cœreba bahamensis*.

But few changes are noticeable among the Mniotiltidæ, and these principally the addition of new subspecies. The Dusky Warbler (*Helminthophila celata sordida*) is one of these, *Dendroica æ. sonorana*, *Geothlypis trichas ignota* and *Geothlypis poliocephala ralphi* being the others.

To the family Troglodytidæ there are to be noted a number of additions and some few changes. They may be shown thus:

1886.		1895.
Harporhynchus longirostris	=	*H. l. sennetti.*
		Subsp. added.
		Harporhynchus cinereus mearnsi.
Genus Campylorhynchus	=	Genus Heleodytes.
C. brunneicapillus	=	*H. brunneicapillus.*
		Subsp. added.
		H. b. bryanti.
714. *C. affinis*	=	Omitted.
		Subsp. added.
		713 *b. H. b. affinis.*
		Catherpes mexicanus punctulatus.
		Thryothorus ludovicianus lomitensis.
		Species added.
		Thryothorus leucophrys.
Thryothorus brevicaudus	=	*T. brevicauda.*

	Subsp. added.
	Troglodytes ædon aztecus.
	Cistothorus palustris paludicola.
	Cistothorus palustris griseus.
	Sp. added.
	Cistothorus marianæ.

In the Certhiidæ, *Certhia familiaris mexicana* becomes *C. f. alticola*, and *C. f. montana* and *C. f. occidentalis* are added as new subspecies.

Among the Nuthatches and Tits (*Paridæ*) the following additions and changes are to be noted.

1886.	1895.
	Subsp. added.
	Sitta carolinensis atkinsi.
	Sitta pygmæa leuconucha.
	Parus bicolor texensis.
	Subgenus PARUS inserted.
	Parus carolinensis agilis.
	Parus hudsonicus stoneyi.
	Parus hudsonicus columbianus.
	Species added.
	Psaltriparus santaritæ.
[745] *P. mlanotis*	= *Psaltriparus lloydi.*

Finally, among the family *Turdidæ*, we have:

1886.	1895.
Turdus f. salicicolus	= *T. f. salicicola.*
Sialia mexicana	= *S. m. occidentalis.*
	Subspecies added.
	Sialia mexicana bairdi.
	Sialia m. anabelæ.

I am now prepared to present some comparisons with respect to the numbers of species and subspecies in 1886 and 1895, and these may be best shown again by means of a Table, as follows:

TABLE.

Group.	Recorded in 1886. Sp.	Recorded in 1886. S. sp.	Recorded in 1895. Sp.	Recorded in 1895. S. sp.
Pygopodes	33	4	32	4
Longipennes	44	4	46	4
Tubinares	31	3	32	3
Steganopodes	17	5	19	5
Anseres	51	6	51	8
Odontoglossæ	1	0	1	0
Herodiones	19	2	20	3
Paludicolæ	17	3	21	1
Limicolæ	66	4	68	6
Gallinæ	22	18	21	22
Columbæ	12	0	11	4
Raptores	53	29	54	37
Psittaci	1	0	1	0
Coccyges	9	0	9	4
Pici	23	11	22	14
Macrochires	26	3	26	7
Passeres	313	117	321	185
Grand total	738	209	755	307

This table will go to show that taking the species and subspecies together in 1886, they amounted to 947, while in 1895 there were no less than 1062. In substracting the number of species recorded in 1886 from those in 1895, we find that there has been a gain of 17 species, and in dealing with the subspecies in the same manner, we find that there has been a gain of 98 subspecies. A study of this table is interesting in other ways, as the making of similar comparisons of any single group, or those groups exhibiting the greatest increase and the causes therefor; but all such data can be easily appreciated by the reader from what has been given above, and my space will not admit of enlarging upon it here.

For a moment we may now turn to the "Hypothetical Lists" of the two editions of the work I have under consideration. In 1886 there were 26 species and subspecies relegated to its hypothetical list, ranging from 1 to 5 for the families in which they occurred. In 1895, *Diomedea exulans* is seen to be added

to the number, while *Chen cærulescens* is considered to belong to our avifauna, and has therefore been added to the list of 1895. The Swallow-tailed Gull, given as *Creagrus furcatus* in 1886, is now *Xema furcata*, and *nine* examples of it are said to be known to science, instead of only *three*, as reported in 1886. *Numenius arquatus* and *Chordeiles v. sennetti* are also added to the hypothetical list of 1895, while *Buteo fuliginosus* is ignored entirely.

Coming at last to the "List of Fossil Birds of North America," we find that as compared with the existing species, a greater number has been added to those previously known than there has been to the list of living birds. In 1886 there were 46 species of fossil birds reported, while in 1895 there were 64 upon the record. No doubt there are others that should have been added to these, overlooked by the Committee, as, for example, the rail-like bird called *Crecoides osbornii* Shufeldt, from the Upper Cenozoic of the staked plains of Texas. Marsh increased the list of Cretaceous Birds by the addition of three species, and the Tertiary Birds by one species, while Shufeldt added no less than fourteen new species of fossil birds as belonging to this latter geological horizon.

To this list should also have been added those belonging to the "Recent Era," as, for example, *Plautus impennis*—the Great Auk—and *Camptolaimus labradorius*—the Pied Duck. Of the first named species there is an abundance of subfossil material in existence, and of the latter there are doubtless bones to be found in the dried skins of specimens in museums and elsewhere. Both birds are quite as extinct as is the famous Jurassic bird, the *Archæopteryx* of the Solenhofen States of Bavaria.

But the addition of new birds to the avifauna of any country is by no means all there is to ornithology. Nor does the science see its end when these new forms have been described, figured and printed in an official list. The importance of giving a new bird a name, recording its superficial characters, and defining its geographical distribution is not to be underrated, the more especially so as all this greatly helps those who are engaged with the science of their morphology, their taxonomy, and their present affinities and past origin. One of the chief

aims of ornithology is to establish the true relations of existing and extinct forms of birds to each other, and to other groups of animals that are either to be found living at the present time, or else have existed during past ages of the earth's history. In other words, the true classification of birds is to be sought for, and ornithology in this sees its most difficult problem and its final goal.

But the knowledge of the origin of this most perplexing group of vertebrates, their evolution, and our power to correctly classify them can only come to us in one way, and that is through a complete understanding of their structure, and a comprehension of the anatomy of those groups more or less nearly related to them. Other departments, however, can lend great assistance here, and the avian taxonomist can have much light thrown upon his arduous task through the revelations of researches in the fields of physiology, of geographical distribution, nidology, paleontology, and other biological sciences.

With these facts before us, it is with no little interest that the taxonomist scans the pages of the second edition of "The A. O. U. Check-List of North American Birds," with the view of ascertaining what evidences there may be in the direction of a better knowledge of the classification of our birds. There may have been some excuse for the numerous symptoms of the somewhat antiquated taxonomy that characterized the arrangement of North American birds in the 1886 edition of the A. O. U. Check-List, but not so this last one, provided we find that the earlier classification has been retained. For, be it known, in the meantime, that is, from 1886 to 1895, the avian morphologists had not been idle. There were very many useful suggestions in the admirable work done by Dr. Stejneger that appeared shortly before the 1886 edition was printed. This was followed, in 1888, by the superb volumes of Fürbringer, with one of the most elaborate classifications of birds the world has ever seen; Seebohm, of England, had done a great deal, while the present writer had published accounts of the osteology of nearly every family of N. American Birds, and Mr. Lucas stands prominent in his excellent anatomical work upon many of the groups. English pens had contributed memoir after

memoir along similar lines, and one has but to turn to the essays and volumes of Newton, Gadow, Beddard, T. J. Parker, Sharpe and many others to appreciate this. But for one to fully know what a deal was done during the nine years I speak of, it is but necessary to read the enthusiastic address of Fürbringer given before the Section for the Anatomy of Birds at the Second International Ornithological Congress, held at Budapesth in 1891. A powerful light has been thrown upon the structure and affinities of the various groups of birds, and has it in any way affected the classification of the 1895 Check-List of North Ameican Birds, that is, in so far as the main groups are concerned? Not in the least. Apart from the addition to the List of the family *Cotingidæ*, the taxonomy of the orders and families as given in 1886 are identical with the arrangement reproposed in 1895. For example, we still find the Grebes, Loons and Auks retained together in the Order PYGOPODES, with the first-named separated from the last two by subordinal lines; whereas, Fürbringer, Thompson, Sharpe, myself and others, all of whom have examined the structure of these birds, have shown the affinity existing between the Grebes and Loons, and that these two families are very distinct from the Auks. The Auks, in fact, occupy a group by themselves, and are more nearly related to the Longipennes. Fürbringer separated them very widely from the Grebes and Loons, in which opinion Sharpe and others concur. That the Longipennes and the Limicolæ are akin is now generally recognized by those who have studied the anatomical structure of the members of the two groups, yet in the A. O. U. classification, six entire Orders stand between the Gulls and the limicoline assemblage. Fürbringer makes a "Gens" Laro-Limicolæ, and Sharpe keeps the two groups close together. As long ago as 1867 Professor Huxley clearly showed the osteological agreement between the skull of a Plover and that of a Gull.

That the Fowls (*Gallinæ*), Pigeons (*Columbæ*), Raptorial Birds (*Accipitres*), Parrots (*Psittaci*) and the Cuckoos (*Coccyges*) as groups should stand in lineal series I can well believe—but as Gadow, Hubert Lyman Clark, myself and others have frequently pointed out, the Owls do not belong with the Acci-

pitres or the Falcons and their kin, while I make separate groups for the Cuckoos, Kingfishers and Trogons. The Woodpeckers are not separated from the Passeres by the Goatsuckers, Swifts, and Hummingbirds, as the A. O. U. List now have them arranged, but the Woodpeckers, in the list of North American Birds, taxonomically arrayed, should stand immediately next to the Passeres, while the " Macrochires " is a thoroughly unnatural group, inasmuch as birds are no longer classified and restricted to groups on account of their having long pinions.

Finally we come to the *Passeres* with the lineal arrangement of the 21 families composing the group. Now, as a classificatory scheme, this lineal method of showing it is unsatisfactory in the extreme, but it appears to be the only available one to adopt in the Lists in books. A " tree " shows what is meant much better and truer, but it can never form a part of a List. Still these Lists show something, for we can, among other things, indicate in them the families that should, in our opinions, occupy the extremes—as, for instance, the *Tyrannidæ* and the *Corvidæ*, but in numerous cases it will be found to be exceedingly difficult to complete the sequence, even to carry out the hopes of the classifier. However, marked violences can usually be avoided, and marked affinities often shown in a classification of this kind.

The scheme adapted in the A. O. U. Check-List, although not altogether a bad one, is capable of showing a more truthful arrangement of the families of passerine birds. In the first place, this List should be completely reversed; then the Thrushes (*Turdidæ*) placed more nearly where they belong; and the *Laniidæ* removed very much nearer the Clamatorial end of the sequence, and away from the Vireos, with which family they have no special affinity. Thus much for the progress in American ornithology during the past ten years; our ornis has been most carefully studied in so far as the identification of new species and subspecies is concerned, but the matter of scientific classification of birds demands increased attention, and it is to be hoped that a greater number of avian morphologists will arise, and should that come about, the clas-

sification of the next edition of the A. O. U. Check-List will, in truth, be archaic if again printed without change; the 1895 one, just out, is a number of years behind the science of the times, so we may easily imagine how very backward it will appear ten years hence.

THE PATH OF THE WATER CURRENT IN CUCUMBER PLANTS.

By Erwin F. Smith.

Although Sachs' notion that the ascending water current in plants passes through the walls of the vessels and not through their interior, was rendered very doubtful long ago, if not thoroughly exploded, by the experiments of Elfving, Vesque, Erera, Boehm and others, the old statement still remains in many of the text books and continues to be taught. For this reason, and because the papers of the opponents of this view do not seem to have received much attention in this country, while Dr. Sachs' *Lectures on the Physiology of Plants* in H. Marshall Ward's admirable translation, is known and read everywhere and deservedly so, it may be worth while to call attention once more to the present state of our knowledge on this subject. This I shall do by presenting some experiments of my own, which were made a year ago on *Cucumis sativus* L. These were undertaken partly to verify some of Strasburger's statements in his book *Ueber den Bau und die Verrichtungen der Leitungsbahnen in den Pflanzen*, and partly to determine, as accurately as possible, the path of the water current in Cucurbitaceous stems, subject to the attack of *Bacillus tracheiphilus*. They were begun about March 20, and continued till some time in April, the weather being by turns warm and cold, sunny, windy, cloudy and rainy. About 30 well grown cucumber vines were experimented upon, the following being selected as typical. All were under glass in a large hot-house, devoted to the cultivation of cucumbers for the winter market. None of the vines trailed on the ground, but all were trained up on

stakes or over high strung wires. A sharp razor was used in cutting the stems.

Before proceeding to the experiments, it will be necessary for the sake of those who are not familiar with the structure of the cucumber stem, to briefly indicate its anatomy. The bundles are bi-collateral, i. e., there is a group of phloem on the inner, as well as on the outer face of the bundle. The outer phloem is separated from the central strand of xylem by a cambium zone, which is restricted to the bundle, i. e., not inter-fascicular. The inner phloem is separated from the xylem, by a meristematic tissue structurally much like cambium, but functionally different. The phloem consists of numerous large sieve tubes, with the usual accompanying cells and cambiform cells. The central or xylem strand of the bundle consists principally of large pitted vessels, held together by shorter tracheids and lignified parenchyma. The mode of origin of the pitted vessels, i. e., out of a series of large superposed cells, is plainly visible, the cross septa being sometimes present and perfect, but more often partially wanting or reduced to mere rims on the inside of a continuous tube. The walls of these tubes contain thousands of very thin places, or actual perforations, (in many cases the central slit takes no stain), and the tubes appear to be admirably adapted for water reservoirs, any adjacent portion of the plant being clearly able to draw from them without hindrance. It appears to me somewhat doubtful, whether they also function as direct water carriers. This business seems more suited to the spiral vessels which occur in a little group on the inner face of the xylem strand, embedded in a delicate, non-lignified living parenchyma, which frequently contains chlorophyll. The walls of these spirals are not pitted; their bore is almost capillary, i. e., much less than that of the pitted vessels; and they are of great length, probably by means of splicings extending as open tubes the whole length of the vine. That they are of more fundamental importance to the plant than are the pitted vessels, appears from the fact, that they are the only tubular parts of the xylem to be found in the smaller roots, and are also the only xylem-vessels passing out of the stems into the peti-

oles and ramifying in the veins of the leaves. It seems to follow from this that whatever be the path of the water current in the stem itself, it can enter the body of the plant in quantities sufficient for transpiration purposes only along the pathway of the spirals, and can reach the leaves only through the same channels.

The pitted vessels are probably sometimes nearly full of water, and at other times nearly empty, the amount depending on the quantity in the soil and on the activity of transpiration. Owing to the number of very thin places or actual perforations in their walls, they undoubtedly contain air at all times and probably often in large quantities. I regard these vessels as water reservoirs. In this capacity they appear to be admirably adapted to serve the needs of a class of plants which (on account of the extent and unprotected nature of their transpiring surface) often make sudden and very large demands on the stem for water,—demands greater than can be met by the immediate activity of the roots. There is, however, nothing against the supposition that when they are not full of water, they may also serve as aerating organs, the stems being alive and chlorophyll-bearing clear to the center. The function of the spiral vessels, according to my conception, is quite different. They also contain a greater or lesser quantity of water, according to the activity of transpiration and the amount procurable from the soil or from the neighboring reservoirs (the pitted vessels), but unlike the pitted vessels, they are surrounded by a living, non-lignified, non-lacunose parenchyma, and there is no free access of air to their interior, but, on the contrary, so far as we can judge from the anatomical structure, this part of the plant has been developed with special reference to keeping it out. When the spirals are not full of water, they probably contain rarefied air. The very thin walls of these spiral vessels bear on their inner face lignified annular or spiral thickenings, which are probably of great service in strengthening the delicate walls, so that they may be strong enough to resist the collapsing tendency of the vacuum pull due to the osmotic pressure, and yet remain thin enough to readily allow water to filter into

them or out, as the case may be. Such, roughly sketched, is the nature of the bundle, the xylem part of which contains 5 or 6 spirals and from 12 to 15, or more pitted vessels. The cucumber stem, exclusive of the hypocotyle, usually contains 9 such bundles, the 5 larger ones forming an interrupted ring or cylinder in the central part of the stem, and the four smaller ones alternating with the larger ones nearer the surface of the stem, the fifth bundle of the outer series being usually wanting in this species. These bundles are separated from each other by thin-walled, living cells which are nearly iso-diametric. The central portion of this parenchyma and that between the bundles, may be designated as medullary tissue, and that farther out as cortical parenchyma, although all of this fundamental tissue bears chlorophyll, and is used to store starch in prior to the development of the fruit. Outside of the bundles, and not far from the surface of the stem, is a compact tissue formed of numerous elongated, thick-walled, flexible, strengthening cells. These are the bast fibres, forming collectively, the stereomatic sheath. This sheath is several rows of cells thick and forms an broken or nearly unbroken cylinder in the young stem, but is afterwards ruptured longitudinally into a dozen or more strands by the growth of the stem in thickness. Between these strands of stereome, the cortical parenchyma finds its way to the epidermis, except where the latter is specially strengthened by sub-epidermal strands of collenchyma. The stem appears to have so developed as to secure every advantage to be derived from a combination of lightness with flexibility and strength.

To indicate the movement of the water in the stems and leaves, various aniline stains were tried, e. g., eosine, soluble nigrosene, methyl green, methyl orange, acid fuchsin, etc. Eosine proved by far the most satisfactory, none of the other stains moving with anything like the same rapidity, and some of them causing copious precipitates in the vessels. None of the substances in the sap of the cucumber vessels cause any precipitate with eosine, and it is probable that dilute solutions of this substance, while clearly poisonous to the plant, move with the same rapidity as pure water, at least at first.

1. Upward Movement of One Per Cent. Eosine Water Through Cut Stems.

(No. 9). This vine was 215 centimeters long and bore a number of small leaves and 17 large ones, 10 of which averaged 20 cm. in breadth. March 21, 2:30 p.m. The stem near the earth was cut under water and put at once into 1 per cent. eosine water.[1] 2:43 p.m. The stain is now distinct in all of the principal veins of a leaf only 15 cm. from the end of the stem, i. e., it has passed up the stem a distance of two meters in less than 13 minutes, probably in 10 to 12 minutes. 2:47 p. m. The red stain is now distinct in the veins of the small undeveloped uppermost leaves of the stem. 3:25 p. m. Slight droop of the foliage, but much less than in No. 10 (a similar vine in 10 per cent. eosine water). Foliage decidedly less red than that of No. 10. 4:35 p. m. Leaves drooping very decidedly. The leaves of No. 10 are flabbier and redder, but much less fluid has passed up the stem. 5:10 p. m. About 21 cc. of the eosine water has passed up the stem in 2 hours and 40 minutes. March 22, noon. Leaves, tendrils and surface of the young fruits reddish. The stain does not make its way readily into the coiled tips of the tendrils. Many of the leaves are dry shriveled, so that they crackle on touch. Stem not shriveled. Most of the petioles are still turgid and but little stain is visible in them, except in a few toward the top of the vine. 4:00 p. m. Not nearly so red as No. 10. Stem quite green and not noticeably shriveled. The stem of No. 10 in the 10 per cent. eosine has shriveled decidedly to-day. March 23, 12:25 p. m. About 10 cc. of the stain has passed up the stem since last night. 4:30 p. m. About 10 cc. of the stain has gone up the stem since the last record. March 25, 12:30 p. m. About 20 cc. of the stain has passed up the stem since the last record. Most of the leaves are crisp dry, but the terminal ones are still moist, although shriveled and soft like old rags, the parenchyma being yellow and the veins bright red. Most of the petioles are bright red, and all of them are limp and hang straight down; the stem has shriveled and become reddish, except the

[1]Distilled water containing Dr. Grübler's "Eosine Soluble in water."

submerged part, which has kept its turgor and resists diffuse staining better than the parts in the air. The plant is dead. March 26, 2:40 p. m. About 12 cc. of the eosine has passed up the stem since yesterday p. m.

In this plant over 40 cc. of the eosine water passed up the stem during the first 24 hours, and in the next four days an additional 45 cc., part of which after the plant was dead.

Vine No. 1 which was 188 centimeters long, also took up the eosine water after it was dead. This absorption of the stain continued long after the leaves had become dry-shriveled, and did not entirely cease until all parts of the bright red stem became bone-dry. This vine was under observation 14 days, during which time about 150 cc. of 1 per cent eosine water passed up the stem, only 57 cc. of which went up during the first 49¼ hours.

(No. 25). This was a young vine, measuring 100 centimeters above the cut surface. It bore 17 leaves, the largest 6 averaging 13 cm. in breadth. March 28, 11:56 a. m. The stem was cut under water and put at once into an alkaline eosine water, made by putting 1 gr. eosine into 100 cc. of $\frac{N}{10}$ caustic soda (the solution stood in the laboratory over night and became darker colored). 12:01 p. m. The red stain is distinctly visible in the veins of all the leaves, even the uppermost ones, i. e., it has gone straight up a distance of one metre in 5 minutes. It is sunny and windy, and transpiration is active. The dry bulb registers 22° C.; the wet bulb 17.3° C. 12:10 p. m. The foliage begins to droop. 12:40 p. m. Foliage wilting very badly. 2:10 p. m. About 5 cc. of the stain have passed up the stem. The lower leaves have begun to crisp at the margin. March 29, 2:30 p. m. About 7 cc. of the stain have passed up the stem since the last record. The blades of the leaves are crisp and the petioles are bright red. March 30. Fluid quite dark ; an additional 4 to 5 cc. has gone up the stem. Stem and petioles much brighter red than yesterday. April 3, 11 a. m. The entire stem and all of the petioles have become extremely bright red, the eosine water (20 cc. of it) having continued to pass up the dead stem since the last record. The leaves appear to have taken up no stain since March 29.

They are not now crisp, but feel limp like old rags. The veins are bright red, but the parenchyma is yellowish-white. The surface of the stem feels moist and stains the fingers red when rubbed.

Similiar results were obtained with a 1 per cent. solution of sodium chloride containing 1 per cent. eosine. Acidulated waters (1 per cent. citric acid and 1 per cent. hydrochloric acid) also passed up the cut stems rapidly and in large quantity, and after the stems were dead. The 1 per cent. hydrochloric acid proved much more poisonous to the plant than did the 1 per cent. citric acid. Similar experiments were made with hydrant water. In the latter, after a few days, the plants reduced their foliage to a minimum, and then lived on for many days, i. e., in case of a plant used for comparison with No. 1, until long after the latter was dead and dry.

To sum up the results of these experiments, of which the preceding are only examples, we have the following propositions:

(1). The rate of movement of the water current in cucumber stems during active transpiration is at least 10 to 12 meters an hour. (2). Absorption of water and transpiration continues in dead stems for some time, i. e., until they have become dry. (3). Large quantities of fluid passed through the cut stems during the first few days. (4). When the cut stems were plunged into water tinged with eosine, sufficient of this stain was taken up to color all the tissues of the plant bright red, including parenchyma, sclerenchyma, collenchyma and epidermis; the first parts to show the stain being the spiral vessels.

(*To be Continued.*)

ON THE MISSISSIPPI VALLEY UNIONIDÆ FOUND IN THE ST. LAWRENCE AND ATLANTIC DRAINAGE AREAS.

By Chas. T. Simpson.

The entire Mississippi drainage area is peopled by a peculiar Unione Fauna.[1]

The species are exceedingly numerous. and many of them attain great size, or become very solid at maturity. A large number are characterized by strong sculpture in the form of knobes, pustules or plications, or by striking outlines, and the species in general are more richly colored externally or internally than those of any other part of the globe.

The Atlantic drainage area, including a considerable part of the St. Lawrence River system, is occupied by a very different Naiad fauna. As a rule the species are moderate in size and conform nearly to the ordinary oval or oblong-oval Unione type; they are of light structure, without sculpture or strong angularities and lobes, and are plain colored in nacre and epidermis.

The dividing line between these two Unione faunas is not directly on the Height of Land, which separates the St. Lawrence and some of the other Atlantic drainage systems from that of the Mississippi, but it is considerably to the northward and north-eastward of it.[2]

To the westward the Red River of the north, the Saskatchewan and Mackenzie are largely inhabited by Mississippi Valley Uniones, and they are found abundantly in all the great lakes, the southern peninsula of Michigan, the streams in Wisconsin, Indiana and Ohio that drain into these lakes, and well up into Eastern Canada, Lake Champlain and

[1] See paper by the writer "On the Relationships and Distribution of the North American *Unionidæ*" in Am. Naturalist, *XXVII*, p, 353.

[2] This matter will be discussed in a paper by the writer, which will soon be published in the Proc. N. S. National Museum "On the Classification and Distribution of the Naiades,"

the Hudson River, in some places mingling with the forms belonging to the Atlantic drainage area proper, in others occupying the waters exclusively.

I think we may safely take it for granted that the only way in which the Mississippi Valley *Unionidæ* could have entered these northern and north-eastern river systems was by migrating along connecting fresh water. As there is no such connection to-day between these systems the question as to how they reached their present distribution becomes an extremely interesting one.

If the theory of the Ice Age as held by most glacialists is a true one I think it will fully explain the present remarkable distribution of these extra-limital Mississippi Valley Naiades. And at the same time I believe the evidence of these fresh water mussels is strongly corroborative of the glacial theory. It is held that at the close of the Ice Age a great cap of ice of immense thickness covered North America east of the Rocky Mountains, down to about Latitude 40°. That with the coming on of warm weather it gradually melted away at its southern extremity, and that when this thawing was continued north of the height of land great lakes were formed whose southern shores were the slope of the land which raised towards the south, and whose northern borders were the slowly dissolving wall of ice. On account of the ice to the northward this water could only drain into the Mississippi system, or to the Southeastward, and several old channels are found through which it is believed that it flowed. One of these is the Red River of the North, which almost connects by means of Traverse Lake at its head with Big Stone Lake at the head of the Minnesota River. There is still a broad channel near the western end of Lake Superior which connects with the St. Croix River, and at Chicago there was no doubt an overflow from Lake Michigan into the Des Plaines River, and Lake Erie is believed to have had its outlet into the Wabash through the Maumee which nearly connects with it. The two streams are connected over a very flat country by an old channel not less than a mile and a half wide, and having an average depth of 20 feet. For 25 miles this character continues, and there is

very little fall either way. To the northeast this channel opens out into an ancient lake, and at the southwest it touches bed rock at Huntington, and then descends more rapidly.[3]

It will be noticed on the map that the St. Josephs, St. Mary's, and Auglaize Rivers, tributaries of the Maumee, flow in the direction of the Wabash, that the two former join at Fort Wayne and flow partly backward as the Maumee; the whole looking like a tree with its branches broken down, and hanging against its trunk. If the river was continued into the Wabash, and the water all flowed to the southwest it would form a natural looking system. It is quite within the bounds of probability that there were old overflows from the St. Lawrence drainage to the eastward of this through the Oswego River into the Mohawk, or by way of the Sorel into the Hudson, and possibly through eastern Lake Erie into the Alleghany system.

Now if the water from this region north of the Height of Land flowed over into the Mississippi drainage area at various places it would be almost certain that the *Unionidæ* of this system would migrate up these overflows and into the northern lakes, that in this region they would obtain a foothold and flourish, for the reason that at the time of their entrance it is quite probable that all freshwater life of this area was destroyed by the grinding and crushing of the great ice cap. It is possible that a few of the Naiades of the eastern drainage system might have survived in the St. Lawrence Valley but it is more likely that such as are now found there have since reached that region by migration from the overflows through the Mohawk and Oswego Rivers, or the Sorel. There has probably been at some time since the close of the Glacial Epoch a connection between the Hudson River and Lake Champlain, as the latter is largely peopled with Mississippi Valley *Naiades.* These forms, most likely, entered Lake Erie through the old Maumee Channel, or by some connection with the Upper Ohio system, passed into Lake Ontario, thence through the Oswego

[3] See a paper "On the Ancient Outlet of Lake Michigan," by Prof., W. M. Davis. Pop. Science Monthly, XLVI, No. 2, p. 217. Also a paper on this old system by G. K. Gilbert, in the first volume of the Ohio Geological Survey.

and Mohawk Rivers into the Hudson, and across into Lake Champlain; or they may have gone down the St. Lawrence and up the Sorel. If by a subsidence since that time Lake Champlain has been connected with the ocean, as is now believed, the Naiads of that lake no doubt retreated up the small streams flowing into it, and returned after the elevation of the land when its waters again became fresh.

I think I am not making too sweeping an assertion when I say that all the Mississippi Valley species of Naiades that have entered the St. Lawrence, or in fact any part of the Atlantic drainage areas, have become changed in some of their characters. As a rule, though not in every case, they have become smaller, and simpler in their outlines; the sculpture is less pronounced or is almost obliterated; in many cases the shells are thinner, the nacre has lost its brilliancy, and instead of the bright epidermis, often painted beautifully with rays or a wonderful pattern of rich greens, yellows, and olives we have mostly dull, livid, ashy or rusty reddish or brownish exteriors, and they are very often somewhat distorted. This is not, as I believe, in any great measure due to climate or colder water, for these same species are as vigorous and finely developed in parts of Wisconsin drained into the Mississippi, Minnesota and Dakota as in any part of their area; besides *Anodonta edentula* under the name of *A. undulata*, and *Unio* (*Margaritana*) *marginata* when found in Maryland, Virginia, and probably even south of that are so dwarfed and stunted as to be scarcely recognizable. This changing of characters has been well illustrated in a lot of *Unionidæ* recently submitted to me for examination by Prof. B. W. Everman of the U. S. Fish Commission, which was collected mostly from the Maumee basin by Dr. Philip H. Kirsch, of Columbia City, Indiana. This region lies in Lat. 41° to 41½°, the most southerly part of the St. Lawrence drainage. *Unio. luteolus* Lam., *U. subrostratus* Say, *U. circulus* Lea, *U. phaseolus* Hild., *U. multiplicatus* Lea, *U. multiradiatus* Lea, and *Anodonta grandis* Say, are so dwarfed and stunted, and changed in color as to be scarcely recognizable, while the same species from the Wabash, from which these have no doubt all been derived, are as vigorous and finely developed as any in the Mississippi Valley.

This great change in size, form and coloring has caused students to bestow many specific names on what I believe are merely northern races or varieties of common Mississippi Valley species. Thus Anthony's *Anodonta subangulata* and Lea's, *A. footiana*, *A. marryattana* and *A. benedictii* are merely dwarfed and slightly changed forms of Say's, *A. grandis*. Anthony's *A. subinflata* is probably a form of *A. corpulenta* Cooper, and *A. subcylindracea* Lea, is the northern manifestation of Lea's well known *A. ferussaciana*. Say's *Anodonta edentula* becomes in Michigan *Alasmodonta rhombica* of *Anthony*, and further east and southeast *A. undulata* of Say; Lea's *Unio circulus* of the central Mississippi area changes in Lake Erie to the dwarf *U. leibi* of the same author; his *U. canadensis* is only an altered over *U. ventricosus* of the western States, and A. Gray's *U. borealis* is a very much changed form of the common *U. luteolus*, while *U. hippopæus* Lea, of Lake Erie is, I believe, only a stunted *U. plicatus* that has almost entirely lost its plications, and has assumed a dirty, reddish or olive color.

Some of these are possibly valid species; most of them would certainly be considered so, together with a number of other northern manifestations of Mississippi Valley species were it not that so many intermediate links are found.

It sometimes happens that specimens of a given species are found in the Mississippi area, growing, no doubt, under unfavorable conditions, that so closely imitate the same species found in northern waters as to be indistinguishable from it. Thus Lea has in his collection what he called *Anodonta footiana*, a Michigan form, from Illinois, and depauperate *Unio plicatus* are sometimes found in the Mississippi area that are almost exactly like *U. hippopæus*. And on the other hand occasionally fine specimens of *Unio rectus*, *U. rubiginosus*, *Anodonta ferussaciana* and *A. grandis* are found in the St. Lawrence drainage that are perfectly normal. Yet as a rule an expert can tell at a glance whether a specimen grew in the Mississippi area or was extra-limital.

Anodonta simpsoniana Lea, is, I believe, a good species, although it is probably an altered and dwarfed *A. grandis*.

It is possible that here we have an opportunity to make some kind of an estimate as to the time required in developing species and varieties among the *Unionidæ.* It is well known that the Laramie strata of the northwest, belonging perhaps to the upper cretaceous or earlier Tertiary systems contain the remains of a large number of Unios which appear to be very closely related to existing Mississippi Valley forms, and are probably their progenitors. Some of these old fossils are so much like certain recent species that they might easily be taken for them by an expert, and nearly or quite all of them can be placed in existing groups.

Yet it is more than probable that the great variety of changes that have been produced in the Mississippi Valley forms which now inhabit the St. Lawrence drainage area have taken place since the Ice Age began to draw to a close, because it is almost certain that all fluviatile and lacustine life under the ice sheet was destroyed, and that any forms closely allied to those of the Mississippi Valley now found north of the Height of Land migrated there since. It is held by most glaciologists, I believe, that the Glacial Epoch reached down probably to within from 10,000 to 20,000 years of the present. This amount of time might probably be taken as the age of these peculiar forms of St. Lawrence Mississippi Naiades.

Unio radiatus, ochraceus, cariosus, heterodon, tappanianus, and *Margaritana undulata,* which are found in the Atlantic drainage south of the line of the ice cap, and which are all closely related to common Mississippi Valley forms are probably older, and may have been derived from some migration made from the western to the eastern drainage at a much earlier date. At any rate I believe that all the Uniones which belong properly in the Atlantic drainage system were derived at one time and another from Mississippi Valley species; that some peculiarity of environment common to this entire region has had a tendency to dwarf them, to simplify their forms and dull their colors.

EDITOR'S TABLE.

Naturalists need not feel unkindly just now towards representative Dingley of Maine, who introduced a bill for the destruction of the seal herd of Behring Sea, which has passed the lower house of Congress. From the point of view of the lover of nature this bill appears to be an atrocity, but everything does not appear on the surface. The sole object is to destroy the commercial value of the herd, so as to put a stop to the slaughter by reckless Canadian poachers. A sufficient number will be preserved to serve as a basis of a new herd, whenever the British and Canadian Governments are ready to join hands with us in the effort to preserve it. The Dingley bill is really a plan for preserving the herd and not destroying it. The fact is that our neighbors across the border have been running up a bill of small accounts against themselves, which will in the aggregate prove burdensome to them some day if continued. It is poor policy for a weak party to make itself unpleasant, especially when the stronger party is desirous of friendly relations. Canadians and Americans are really one people, and we ought to combine not only to protect the seals, but to increase theirs numbers, and develop the industry which depends on them.

Some naturalists think it is quite the proper thing to protest that it is of absolutely no importance whether they receive credit for a discovery or not, and it is more than intimated in print from various quarters from time to time, that interest in such questions is quite inconsistent with the lofty aims of science. We must confess to having become somewhat weary of this alleged elevation of sentiment, for we find human nature to be in scientific investigators not so very different from that which is common to the rest of mankind. Under the circumstances these protestations savor of cant. The naturalist like other men must live. In order to live he must be known; hence necessity forbids that he hide his light if he have any, under a bushel. And in fact the majority of naturalists do not do so. They understand the value of honest advertising. The product of a laborer should be labelled, first for his own advantage, and second for the information of others, who know his personal equation. What we want is honest goods with honest labels, and for these no protestations of pseudomodesty, or depreciation on the part of unpractical idealists, is in place.

We are pleased to notice the excellent scientific work which is being done by the Field Museum of Chicago. The management has called

to its aid a number of able scientific men, and is publishing the result of their work in suitable style. The papers of Hay on the Vertebral Column of Amia, and the skeleton of Protostega, are important contributions to knowledge. We hope soon to give an abstract of the illustrated paper of Holmes on the Yucatan ruins. It seems that the Museum is not to be merely a show place, but is to be a center of original research, worthy of the great city in which it is sitnated.

Perhaps a year ago we objected in rather caustic terms to the proposed publication by the Filson Club of Louisville, Kentucky, of the life and bibliography of Rafinesque. We are at the time under the impression that the club was a scientific body, and we were then of the opinion, as we are now, that such a society might easily find better use for its money than the publication of such a work. The fact is, however, that the object of the society is the preservation of historic records, and not of the results of scientific research. Hence the publication in question was precisely within its scope, and Prof. Call, the author, conferred a benefit on us all in writing the book. The history is a very curious one, and will interest even the non-scientific reader. Manuscripts in the possession of the U. S. National Museum show that Rafinesque had a skillful pencil, and that the figures which accompany his printed works do him injustice.

President Cleveland deserves well of his fellow countrymen for various reasons, but he deserves least, of his scientific constituency. His latest appointment, that of the U. S. Commissioner of Fish and Fisheries, was made in spite of different recommendations of the scientific men of the country, and for reasons which are to this class quite inscrutable. The new appointee was, as we are informed, retired from the navy on account of rheumatism. He has no scientific knowledge or experience of the habits of fishes or the conduct of fisheries, and would seem to be physically incapacitated from learning. Doubtless the President has told him as the old lady told her daughter who asked her if she might go in to swim; father may I the fishes save from thoughtless cruel slaughter? yes, yes my son, save every one, but don't go near the water.

RECENT LITERATURE.

Geological Survey of New Jersey.[1]—The Annual Report of the State Geologist for the year 1894 contains an account of the progress made in the study of the surface geology, by R. D. Salisbury; a report on the artesian wells in southern New Jersey, by L. Woolman, and a statement of the results of the surveys made with reference to ascertaining the forest area of the state, by C. C. Vermeule.

Mr. Salisbury makes an especial point of the influence that "stagnant ice" has had upon the deposition of the stratified drift of the valleys of the northern part of the state. In his description of Flat Brook Valley he remarks that "the form of topography characteristic of this valley, and of stagnant ice deposits in general, is the following: A broad and somewhat swampy flood plain in the axis of the valley is bordered on one or both sides by a strongly-marked kame belt a few rods in width. This kame belt is lowest near the axis of the valley. It rises in the opposite direction, and finally grades into a flat-topped terrace." These terrace differ from normal river terraces primarily in the fact that the slopes which face the axis of the valley are not erosion slopes.

Mr. Woolman's report confirms the conclusions of former observations, that the principal water-bearing horizons are found in Cretaceous strata.

The forestry report includes a paper on the forest conditions of south Jersey, by John Gifford. The interest of this paper centers in the practical suggestions it contains as to the treatment of forest lands, both for their preservation, and for pecuniary return for money and labor spent in their care. The paragraphs on Forest Influences, and Forest Economics should, in the interest of the people, be quoted in every local paper of the State.

Nine page-plates are used for illustrations, and a geological map of the valley of the Passaic—topographic sheet 6 in envelope, accompanies the Report.

Annual Report, Vol. VI, Geological Survey of Canada.[2]—This volume comprises the summary reports on the operations of the

[1] Annual Report of the State Geologist of New Jersey for the year 1894. Trenton, N. J. 1895.

[2] Annual Report (new series) Geological Survey of Canada, Vol. VI., 1892-93. Ottawa, 1895; Dr. H. R. C. Selwyn, Director.

survey for the years 1892 and 93, by the Director; reports on the Geological investigations conducted in central Ontario and southwestern Nova Scotia by F. D. Addens and L. W. Bailey respectively; a contribution to the knowledge of the minerals of Canada, as shown by chemical analyses, by G. C. Hoffman; and a report on mineral statistics and mines, by E. D. Ingall and H. P. H. Brumell.

The Director's report includes much valuable information concerning the hitherto practically unexplored regions of the Labrador peninsula, and the western coast of Hudson's Bay.

Sketch maps of southern Keewatin, and of the south-western part of Nova Scotia accompany the reports on those regions, and a number of statistical diagrams show the progress of the mining indutries.

Elementary Physical Geography.[3]—A new text book of physical geography has been long needed, so that this work of Mr. Tarr's is well timed. The author divides the subject into three parts the Air, the Ocean, the Land, giving the physiographic side more prominence than is customary in works of this kind. The language is clear, the illustration apt, and the information up to date. Each chapter is supplemented by a list of reference books and an appendix contains descriptions of meteorological instruments, apparatus and methods of use, suggestions to teachers, and questions upon the text.

The text is usually well illustrated with diagrams and reproductions of photographs many of them new, while the addition of 29 plates and charts completes a most attractive volume. We can recommend it for use as the best text book for colleges before the public.

Guide Zoologique.[4]—A reference book, published for use during the meeting of the International Congress of Zoology at Leyden in 1895. Brief accounts are given of the zoological courses offered in the various schools of Holland, also of the Zoological institutions, gardens, and societies. The fauna of the country is summarized by specialists, the history of the domestic animals revewed, and a short account of the fishing industry closes the zoological part of the volume. The final chapter is devoted to the climate of Holland.

The many maps and plates which are distributed through the book, its convenient size, and the clear, concise language of the text, combine to make an admirable guide book,

[3] Elementary Physical Geography. By R. S. Tarr. New York and London, 1895. Macmillan & Co.

[4] Guide Zoologique. Communications diverses sur les Pays Bas. Leyde, Septembre, 1895.

Marshall and Hurst's Practical Zoology.[5]—The fourth edition of this work being called for, the work of revising and editing it has devolved upon Mr. Hurst, to bring the work up to date numerous changes have been made, the most important of which, perhaps, are in the chapter on Amphioxus.

The work as originally written was intended to give the junior students of Owens College, Manchester, England, a practical acquaintance with animal morphology, and the present revised edition will be found a useful laboratory text book for any one who wishes to acquire an insight into the leading facts of Animal structure, and a technical knowledge of the principal methods of research.

The illustrations are intentionally few, as it is expected that the student will make drawings from his own dissections. These are, however, of excellent quality.

Works of this class are of utility in the laboratory, but they do not take the place of general text books as guides to the larger problems of zoology.

Elementary Lessons in Zoology.[6]—In the hands of a competent teacher this book will be of value in giving a student a fair start in the study of zoology. It is in reality a Laboratory Manual. Four simple types of animal structure are given to familiarize the student with the meaning of the terms, *cell, protoplasm, tissue, differentiation sexuality,* etc. Considerable attention is given to insects; then follow in turn common forms of Crustaceans, Worms, Molluscs and Vertebrates. The study of the animal alive, and in its biological relation to its environment, is made a prominent feature. To this end methods of observation are given with suggestions as to the facts to be ascertained. In this way the student acquires a practical knowledge of the life histories of the animals studied.

An appendix contains directions for the preparation of material for study.

The illustrations are intended as guides to identification, and in a very general way, they answer the purpose.

Chats about British Birds.[7]—The depiction of bird life in this volume is quite a vivid and interesting as was that of insect life, by

[5] A Junior Course in Practical Zoology. By A. Milnes Marshall and C. Herbert Hurst. Fourth Edition revised by Mr. Hurst. New York, 1895. G. P. Putnam's Sons.

[6] Elementary Lessons in Zoology. By James G. Needham. New York, 1895. American Book Co.

[7] Chats about British Birds. By J. W. Tuft, London, Geo. Gill & Sons.

the same author, in *Rambles in Alpine Valleys.* Members of thirty three families are described in an easy, gossipy fashion, with special reference to their food and nesting-habits. No opportunity is lost for pointing out that in general, birds are the farmers best agents for protecting crops from insects and worms. The fruit eating proclivities of the Thrush and the Black bird in the late summer are excused for the wholesale destruction in early spring of insects, worms, slugs and snails.

The book is intended to interest young people in the study of Ornithology, but from the facts set forth, it may also be of use in creating among farmers a better appreciation of the service rendered them by birds, and lead them to see the necessity of organized protection for the feathered race.

Check List of North American Birds.[8]—The American Ornithologist's Union have issued a second edition of the Check-list published in 1885. The new addition includes the numerous additions and nomenclature changes made in the several supplements to the Check List since the publication of the original edition, together with a revision of the "habitats" of the species and subspecies, but omitting the Code of Nomenclature.

Species whose status as North American birds is doubtful are listed separately under the heading "Hypothetical," and the fossil birds are likewise separately classified.

As an authoritative nomenclator this book has much value, but it could be rendered more authoritative if the A. O. U. would insist on correct orthography in all cases where this is ascertainable. In several instances the list adheres to obvious misspelling and typographical errors; such as hasitata for hæsitata; cincinatus for cincinnatus; Leptatila for Leptoptila; Ammodramus for Ammodromus, etc.; Greek spellings instead of Latin are retained wherever the original authors used them, and some bad examples of the *vox hybrida* are perpetuated.

RECENT BOOKS AND PAMPHLETS.

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[8] The A. O. U. List of North American Birds. Second Edition. New York, 1895.

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General Notes.

PETROGRAPHY.[1]

Ancient Volcanics in Michigan.—In an area in Michigan covered by Townships 42 to 47 N. and Ranges 30 to 34 West, is a succession of granites and gneisses overlain by a thickness of some 3000 feet of volcanic rocks, embracing acid and basic flows and tuffs. Among the basic rocks Clements[2] finds porphyrites and melaphyres, and among the acid ones quartz-porphyries and devitrified rhyolites. The melaphyres and porphyries are described under the names apo-basalts and apo-andesites, because they are altered forms of basalts and andesites. Some of the andesites are amygdaloidal, and nearly all show the effects of pressure. Andesitic and basaltic tuffs are both present. They exhibit no special peculiarities. The quartz porphyries among the acid flows are notable for the existence in them of corroded phenocrysts of quartz in which there has been developed a well marked rhombohedral cleavage. The groundmass of these rocks is sometimes micro-granitic and at other times is micro-poicilitic. The latter structure is peculiar in that it is produced by a reticulating net work of uniformly oriented quartz, between the meshes of which are irregularly shaped areas of orthoclase. The other acid lavas and the acid tuffs are similar to corresponding rocks elsewhere. The series is interesting as affording another illustration of a typical volcanic series of Pre-Cambrian age. It is one of the oldest accumulations of volcanic debris and lavas thus far described.

Gneisses of Essex Co., N. Y.—In a recent bulletin on the geology of Moriah and Westport Townships. Essex Co., N. Y., Kemp[3] gives a general account of the petrography of the gneisses, limestones, black schists, gabbros, anorthosites and dyke rocks of these regions. Most of these rocks have already been described in more detail in other papers. The gneisses are of several varieties. The most common is a member of the basement complex underlying the other rocks of the district. It is a biotite gneiss composed of quartz, micro-perthite, orthoclase, plagioclase and brown biotite, all of which minerals exhibit evidences of dynamic metamorphism. Near iron ore bodies the gneiss becomes more basic, abundant green or black hornblende, green

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Journal of Geology, Vol. III, p. 801.

[3] Bull. N. Y. State Mus., Vol. 3, No. 14, 1895, p. 325.

augite and a large quantity of plagioclase taking the places of the usual gneissic constituents.

Volcanic Rocks in Maine.—In a preliminary notice on the rocks of the Flox Islands, Maine, G. O. Smith[4] gives a brief account of the association of lavas and breccias on North Haven and Vinal Haven Islands. On North Haven the series consists of beds of porphyrites and of coarse volcanic breccias and conglomerates, layers of tuffs and sheets of quartz-porphyry. The porphyrites are sometimes olivinitic. The conglomerates and breccias are composed of fragments of the porphyrites cemented by a porphyritic matrix. The quartz-porphyry possesses no unusual features. On Vinal Haven the rocks are predominantly acid, comprising many banded and spherulitic felsites that were originally glassy rocks. The spherulites are felsitic or fibrous and are certainly original structures, since transitions from the felsitic into brecciated rocks may be traced, in the latter of which occur spherulites that were formed prior to the brecciation. The acid layers of the series are younger than the basic beds.

Spotted Quartzites, S. Dakota.—The Sioux quartzites in Minnehaha Co., S. Dakota, grade upward into variously colored quartz slates that are composed of quartz grains, iron oxides and mica in an argillaceous matrix that has crystallized in part as sericite, kaolin and chlorite. Many of the slates are marked by spots that are lighter than the body of the rocks. These spots are essentially of the same composition as the groundmass in which they lie, except that they contain less iron oxide. Their lighter color is due to bleaching out of the iron salt through the acid, probably of decomposing organic matter.[5]

The Gneisses and 'Leopard Rock' of Ontario.—The gneisses interstratified with the limestones in the Grenville series, north of Ottawa, Canada, vary much in character.[6] The predominant variety is a granitoid aggregate of reddish orthoclase and grayish-white quartz, a little or no mica, and sometimes garnets. Its bedding is very obscure. When the mica is abundant in the rock foliation is distinct. One variety of the rock is called by Gordon syenite-gneiss. It includes the 'leopard rock' of the Canadian geologists. The rock occurs as dykes cutting quartzites and pyroxenites. All the phases of the gneisses show the effects of pressure. The 'leopard rock' consists of

[4] Johns Hopkins Univ. Circulars, No. 121, p. 12.
[5] Beyer: Ib., No. 121, p. 10.
[6] Bull. Geol. Soc. Amer., Vol. 7, p. 95.

ellipsoidal or ovoid masses of feldspar and a little quartz, separated from each other by narrow anastomosing partitions of green interstitial substance composed of pyroxene and feldspar. When the ellipsoids are flattened by foliation the rock becomes a streaked gneiss. Under the microscope, in sections of the coarse grained gneisses, large crystals of pyroxene, microcline and quartz are seen to be imbedded in a fine grained aggregate of microcline and quartz. In the ellipsoidal varieties the ellipsoids are composed mainly of microcline grains and the interstitial mass is a fine grained mosaic of feldspar, quartz and augite. In the streaked gneiss the augite is partially changed to green hornblende, while crystalloids of idiomorphic hornblende indicates that some of this component is an original crystallization. The rocks are evidently sheared pyroxene-syenites. The author discusses the use of the term 'gneiss' and suggests that the term 'gneissoid' be restricted to the description of foliated eruptive rocks whose structure is due to magma motions, that 'gneiss' be used as a suffix to the name of any rock that has assumed the typical gneissic structure since its original consolidation, as diorite gneiss, etc., and that the ending 'ic' be used with reference to the mineralogic composition of a foliated rock whose origin is unknown—a dioritic gneiss, in this sense indicates a folaited rock whose present composition is that of a diorite.

Petrographical Notes.—In thin sections of sandstone inclusions that have been melted by basalts, Rinne[7] finds the remains of quartz grains surrounded by rims of monoclinic augite, cordierite, spinel, etc. In some of the glasses formed by the melting of the sandstone are trichites and crystallites of orthorhombic pyroxene. While this substance is found abundantly as a contact mineral in the sandstones enclosed in the basalts of Sababurg, the Blauen Kuppe and Steinberg, the author nevertheless regards it as a comparatively rare product of the contact action between these two rocks.

Bauer[8] declares that the rubies, sapphires, spinels and other gem minerals from northern Burma occur in a metamorphosed limestone on its contact with an eruptive rock whose nature is not known.

Penfield[9] obtains a heavy solution for the separation of mineral powders whose densities range between 4.6 and 4.94 by melting together silver and thallium nitrates in different proportions. The molten mass

[7] Neues Jahrb. f. Min., etc., 1895, II, p. 229.

[8] Sitzb. d. Ges. z. Beförd der gesamnet Naturw. Marburg, 1896, No. 1.

[9] Amer. Journ. Sci., Dec., 1895, p. 446.

attacks sulphides, but otherwise is of much value in separating mixtures of heavy minerals.

La Touche[10] describes an apparatus to be used in connection with diffusive columns of methylene iodide for the purpose of determining the density of minute fragments of minerals.

GEOLOGY AND PALEONTOLOGY.

Geology of the French Congo.—The Congo region belonging to France is greater in extent than the parent country. Through the enterprise of different explorers and the researches of scientific men, notably geologists and paleontologists, much information concerning this great tract of country has been acquired since 1871. M. Barrat has systematized the facts on record and publishes an interesting paper under the title " Geology of the French Congo," in which he embodies also the results of his own explorations in the valley of the Ogoove.

The observations of M. Pouel, made during a stay of many years in the region under discussion, confirm the conclusions of M. Barrat as to the great stratigraphic uniformity of the Congo. Furthermore, the formation of the basin after the uplift of the African plateau is explained by the progressive draining of a series of reservoirs, more or less depressed, placed at different altitudes and discharging from one to another toward the ocean. The limits of these reservoirs are in relation to the ancient rock ridges now hidden beneath the sandstone but occasionally laid bare by erosion.

Around the border of the basin, the formations plainly demonstrate that they were elevated as early as the carboniferous epoch, and although greatly leveled since, still show the primary reliefs. One of the most interesting is the formation of Adamaona ; its substratum is granitic and metamorphic like the Crystal Mountains and the Mouamba Mountains and the region of Katanga, and there are also rocks which are similar to the Devonian of the Lower Congo. The whole formation has been intersected and to a great extent covered by erupted material, probably Cenozoic, and by outflows from volcanoes of undoubtedly much more recent age.

The structure of the plateaus of Adamaona is somewhat analogous to that of the central plateau of France. (Extr. Ann. des Mines liv. d'Aril, 1895.)

[10] Nature, Jan., 1896, p. 198.

Geology of the Nile Valley.—In a paper on the geology of the Nile Valley, Prof. E. Hull calls attention to the two great periods of erosion in this region, the first during the Miocene period, after the elevation of the Libyan region at the close of Eocene times, and the second during a "pluvial" period extending from late Pliocene times into and including the Plistocene. In the second part of the paper the terraces of the Nile Valley are described and full details given of the characters of a second terrace, at a height varying from 50 to 100 feet above the lower one, which is flooded at the present day. This second terrace is traceable at intervals for a distance of between 600 and 700 miles above Cairo. Two old river channels are also described, one at Kom Ombo and the other at Assuan itself. The author discusses the mode of origin of the second terrace and the old river valleys, and believes them to be due to the former greater volume of the river and not to the subsequent erosion of the valley. He gives further evidence of the existence of meteorological conditions sufficient to give rise to a "plnvial" period, and points out that other authors have also considered that the volume of the Nile was greater in former times. (Nature, March, 1896.)

The Antarctic Continent.—Mr. C. Hedley has published the data to date concerning the forms of life held as common stock by the converging land masses of the southern continent, together with the conclusions reached by several naturalists as to a former antarctic land area and the continuity of the southern land masses. The author states that the evidence collected tends to show Antarctica as an unstable area, at one time dissolving into an archipelago, at another resolving itself into a continent. From the distribution of the pond snail Gundlachia, he argues a narrow land connection during Mesozoic time between Tasmania and Terra del Fuego across the south pole, and that New Zealand at that time reached sufficiently near to this Antarctic land to receive by flight or drift many plants and animals (Proceeds. Roy. Soc. N. S. Wales, 1895).

Two Epochs in Vegetable Paleontology.—A late number of Science contains a tribute by Lester Ward to the memory of two eminent paleontologists, Marquis de Saporta and Professor William C. Williamson. In the author's judgment, the most important contribution of the former to science is the conclusion that the most important subdivisions of the geological scale must be drawn at different points for plant development from those at which they are commonly drawn for animal development. For example, the Mesophytic age properly

ends with the Jurassic instead of with the Cretaceous, while the tertiary for fossil plants closes with the Miocene instead of with the Pliocene.

Of the many important discoveries made by Williamson, the most valuable is the demonstration of the existence of exogenous structure in the Carboniferous Pteridophytes. (Science, Vol. II, 1895.)

The Appalachian Folds.—The faults and folds in Pennsylvania Anthracite beds are most admirably shown by Dr. Benjamin Smith Lyman in a paper illustrated by thirty-three page plates containing 177 sections. These sections were prepared by the author from the valuable cross-section sheets of the State Geological Survey, and are accompanied by a key map showing where the sections are made. From a comparison of these cross sections Dr. Lyman draws the following conclusions:

"Steep northerly dips in the Pennsylvania anthracite region are much less prevalent than was formerly supposed; nearly half the basins and saddles are about symmetrical; nearly three-fourths of the subordinate ones are so in the Western Middle field; less than quarter of the main ones are so in the Southern field. Again, the subordinate folds throughout the region are confined to subordinate groups of beds of inferior firmness, and are not parellel to the main folds, but probably at uniform profile distances from the main axes, so as to descend the flanks of a sinking anticlinal. Further, that the faults are most invariably longitudinal or reversed faults, occasioned by the overtraining of subordinate folds, and corresponding in three-fourths of the cases to an overturned southerly dip, with the upthrow to the south; such broken subordinate folds, whether dipping southerly or northerly, ride in equal number on the northerly dipping and southerly dipping sides of the main folds; the stratigraphic throw averages only about 62 feet, and never exceeds 160 feet; the displacement averages 72 feet, and never exceeds 240 feet." (Trans. Amer. Institute Mining Engineers, 1895.)

The Ancestry of the Testudinata.—In the Naturalist for 1885, I advanced the hypothesis that the order of the Testudinata arose from the Paleozoic order of the Theromora. In the latter I included at that time the forms I afterwards distinguished as an order under the name of Cotylosauria. In 1892 (Transac. Amer. Philosoph. Society, p. 24) I specified that the Testudinata must have been derived from this latter order. It is now possible to bring positive evidence that this view is correct, since the anticipation so expressed is

now verifiable. Parts of the skeletons of a new form of Cotylosauria from the Permian bed has come into my hands, which represents a new family of the order, and one which may well have been ancestral to the Testudinata. (See NATURALIST, 1896, April for a description of the order). The family may be defined as follows:

OTOCŒLIDÆ fam. nov. Cranial roof excavated laterally behind, forming a large meatus auditorius. Teeth present, in a single row, not transversely expanded. Ribs immediately overlaid by parallel transverse dermoössifications, which form a carapace.

To this family I refer two new genera, viz., Otocœlus and Conodectes, which differ as follows:

Suspensorium directed anteriorly, except at free extremity; nostrils lateral; *Otocœlus.*

Suspensorium directed posteriorly; nostrils vertical; *Conodectes.*

OTOCŒLUS, has the following characters: Intercentra present. Teeth subconical. Mandible not projecting beyond quadrate. Clavicle expanded at both extremities, overlapping the episternum. Scapula with a proscapular lamina. Ribs transversely expanded, not united by suture with each other, alternating with the dermal bands. Limbs well developed.

The type species of the genus Otocœlus is the

OTOCŒLUS TESTUDINEUS sp. nov. The skull is short wide and flat, and the orbits are large and are situated near the auricular excavations. Surface roughly sculptured with small pits and ridges. Malar and mandibular bones shallow. Teeth small, com pressed conic smooth, and without serrations. Scapular arch without sculpture of the inferior surface. Humerus with widely expanded head and narrow shaft. Bands of carapace of moderate transverse extent, and roughly sculptured with pits and tubercles, Width between auditory meatuses 74 mm.; do. between orbits 32 mm.; do. between auditory sinus and orbit 16 mm.; transverse diameter of orbit 30 mm.; depth of mandibular ramus below middle of orbit, 28 mm.; width of carapace 80 mm.; length of clavicle 80 mm.; transverse width of head of humerus 35 mm.; length of femur 67 mm.; length of vertebral centrum 10 mm.; width of do. 19 mm.; width anterior rib distally 11 mm.

A second species is the *O. mimeticus* Cope. But one species of Conodectes is known, the *C. favosus* Cope.

This form is of especial interest since it constitutes, with the genus Dissorhophus which I described in the NATURALIST for 1895, p. 998, a remarkable example of homoplassy. It is doubtful whether the carapaces of the two forms could be distinguished externally, but Dissorhophus is a Stegocephalian Batrachian with rhachitomous vertebræ,

while the present form is a Cotylosaurian reptile. Although so similar superficially, the carapaces of the two differ as follows. In Dissorhophus transverse expansions of the neural spines of the vertebræ support the transverse dermal bands below, and the ribs are free, and only reach the border of the carapace by their extremities. In Otocœlus the neural spines are not expanded, and the dermal bands rest immediately on the ribs.—E. D. Cope.

The Extent of the Triassic Ocean.—In a short note contributed to the Paris Academy of Sciences on December 30, Prof. Iness calls attention to the striking geographical results of the researches of his Vienna colleagues on the marine Triassic fauna. While to English geologists the Trias is the typical example of an unfossiliferous land deposit, the work of Mojsisovics on the contemporaneous deposits of the Alpine region has been the starting point for a series of discoveries in many parts of the world. A rich marine Triassic fauna is now known, extending from Spain to Japan and California, and from Spitzbergen to New Zealand. Yet among the thousands of these fossils gathered together in Vienna from all parts, there is not a single marine fossil from the regions bordering the Atlantic or Indian Oceans. The conclusion is obvious, thas the regions of these modern oceans were not covered by sea in Triassic times. On the other hand, all the districts bordering the Pacific and Mediterranean yield the marine forms, as does a great stretch of land extending from the Mediteranean to the Pacific through Central Asia, and another extending from the Pacific through Eastern Siberia to the Arctic Ocean. Thus the Pacific Ocean was the main ocean in Triassic times, and stretched out two arms across the continental region—the one called the Tethyan ocean, of which the Mediterranean is the last remnant, the other, the Arctic branch. This distribution of the Triassic seas strikingly agrees with that of the structural features of modern coast lines indicated by Neumayr: the oceans bordered by lands with marine Trias are the oceans of the *Pacific type*, of which the coasts are determined by the convex margins of earth folds; while the oceans of *Atlantic type*, of which the margins cut across the mountain folds, are those around which the only fresh water Triassic strata are found. Thus is confirmed the opinion that the latter oceans are of comparatively recent origin, and have been produced by a process of wholesale depression, which has cut off the three great triangular upstanding masses (or *horsts*) of Greenland, Africa and India, which form so striking a feature on the surface of our planet. (Nature, Jan., 1896.)

The Ancient Beaches of Erie and Ontario.—In his correlation of the moraines of western New York with the Raised Beaches (Crittenden and Sheridan) of Lake Erie, Mr. Leverett makes the following statement in regard to the Lake Outlets:

"The evidence is clear that during the formation of the upper two beaches an outlet was found to the Wabash, past Fort Wayne, Indiana. At the time the third or Belmore beach was formed (and its probable continuation, the Sheridan beach) this outlet had been abandoned. It is thought that the ice sheet had retreated so far from the Huron and Michigan basins as to open a lower outlet through these basins than past Fort Wayne. It seems improbable that an eastward outlet was then open, for the district south of Lake Ontario was apparently still occupied by the ice sheet. It is evident that no outlet to the east could have existed until the ice sheet had withdrawn from the Lockport moraine sufficiently for a passage eastward along its southern margin. If my interpretations are correct, the Crittenden beach had been for a long time occupied by the lake before an eastward outlet was opened, a time sufficient, not only for the Lockport moraine, but for several other slightly older minor moraines to be formed. During that time, the lake in all probability discharged westward through the Huron and Michigan basins past Chicago. When the gates to the eastward were opened by the withdrawal of the ice sheet there was probably a brief period in which the lake discharged through the Seneca Valley into the Susquehanna. But soon the lower outlet by the Mohawk was opened and the lake fell rapidly to that level, leaving but feeble traces of beach or wave action in its intermediate stages." (Amer. Jour. Sci., Vol. L, 1895.)

Geological News.—GENERAL.—The periods of volcanic activity in New Brunswick, as stated by Wm. D. Matthews, are:

1. *Huronian.*—Southern New Brunswick and the northern watershed.

2. *Silurian and Early Devonian.*—Passamaquoddy Bay, Baie Chaleur, etc.

3. *Sub. Carboniferous.*—Borders of the central plain, Grand Lake, Blue Mountains of the Tobique.

4. *Triassic.*—Quaco, Grand Manan. (Bull. Nat. Hist. Soc., New Brunswick, No. XIII, 1895.)

PALEOZOIC.—The "shists lustrés" of Mont Jovet are shown by Dr. Gregory to be older than the Trias (1) by the occurrence of fragments of the schists in the Trias; (2) by the discordance of strike between the two series; (3) by the occurrence of masses of dolomite resting unconformably on the flanks of the shists; and (4) by the fact that the Trias has escaped the metamorphism which the schists have undergone. (Quart. Journ. Geol. Soc., 1896.)

Mr. C. H. Gordon's investigations of the St. Louis and Warsaw formations in southeastern Iowa furnishes evidence in favor of Calvin's

conclusion that dolomites are essentially offshore products. (Journ. Geol., Vol. III, 1895.)

CENOZOIC.—Certain data accumulated by H. B. Kümmel indicate the glaciation of Pocono Knob and Mts. Ararat and Surgar Loaf in Wayne County, Pennsylvania. It has been hitherto held by the Pennsylvania State Geologists that these peaks were nunataks. (Amer. Journ. Sci., 1876.)

According to Dr. Grossmann, glacial phenomena in the Færoes comprise roche moutounées, glacial striæ, glacial mounds and boulder clay. The author states that there is no doubt that the islands had a glaciation of their own, a conclusion which is inconsistent with the hypothesis of a big northern ice cap. (Geog. Journ., Vol. VII, 1896.)

M. Harlé has identified the canine tooth and phalangeal bones of a lion, two molar teeth of a reindeer, and a molar tooth of an elan (Alces) in the fragmentary specimens from the Tourasse caverns in the southwestern part of France. From the evidence of other remains, M. Regnault fixed the age of this cave as intermediate between late Paleolithic and Neolithic. The presence of lion remains at Tourasse shows that this carnivore lingered long in the Pyrenees. (L'Anthropol., 1894.)

CAVE EXPLORATIONS IN TENNESSEE.—We discovered the tapir-peccary layer in the cave breccia together with a later fauna in a layer of cave earth, associated with Indian remains, in Zirkel's Cave near Mossy Creek, east Tennessee. Prof. Cope, our informant, had previously found the former in 1869.—H. C. MERCER.

BOTANY.[1]

The Conifers of the Pacific Slope.—Every botanist who is interested in the Conifers (and what *botanist* is not?) will be pleased with the pocket edition of Mr. J. G. Lemmon's "Hand-book of West American Cone-bearers," which appeared somewhat less than a year ago. It is a duodecimo volume of about a hundred pages, and includes seventeen half-tone plates, from photographs, of the foliage, cones, and other characteristic features. The text consists of brief descriptions of the genera and species, interspersed with notes, discussions and narra-

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

tion. In a prefatory note we are informed that this is but a prodrome to a complete work which the author has in preparation, in which full scientific and popular descriptions are to be given. The little volume before us with its modest price of but one dollar should find its way into the library of every botanist, and all will look with expectation to the completion of the larger work.—CHARLES E. BESSEY.

Still another High School Botany.—It will not be the fault of the book-makers if the young people of the country are not versed in Botany, for one scarcely takes up a scientific journal nowadays without finding an announcement of some forthcoming book, or of one just issued. It is a sign of much botanical activity in the public schools, for it is very certain that the publishers are bringing these books out in response to what they regard as a sufficient demand. The last one on our table is the Elements of Botany prepared by J Y. Bergen, instructor in Biology in the English High School of Boston. It is, we are told in the preface, "for the most part an expansion of the manuscript notes which have for some years formed the basis of the botany teaching in the Boston English High School." The book is thus to a large extent a growth; and not a creation. It looks usable, and what is more it has every appearance of being a profitable book to the user. An importance feature of the work is found in its many physiological experiments and observations which are to be made by the pupil. The whole work has a strong physiological bias which will be of much value in leading the pupil to the study of the plant in action, rather than to the identification of species.

Still with all its excellence the book presents the elements of botany in a fragmentary way. After over two hundred pages given to flowering plants, we find but twenty-seven pages given to "Some Types of Flowerless Plants." The pupil will imbibe the notion from this book that the flowerless plants are of less importance than those which receive so much more attention. The book should be called the Elements of the Anatomy and Physiology of the Flowering Plants, and thus restricted it is admirable; but the author was not warranted in calling it the elements of Botany, that is of the whole science, for it certainly does not present the elements of the *science* of Botany. We are glad to note in the very much abridged Flora at the end of the book a departure from the usual sequence of families, but we regret to see that the Gymnosperms, while given their proper place below the Angiosperms, are described in accordance with the old views as to their morphology. When we describe the staminate cones of the pine as catkins of monandrous flowers, and the ovaliferous cones as catkins of

"spirally arranged carpel scales," we must be consistent, and put the Gymnosperms where Bentham and Hooker, Gray and Watson put them, as the simplest of the Apetalæ.—CHARLES E. BESSEY.

Popular Botany.—We frequently deplore the ignorance of people in general as to the main facts of botanical science, and sometimes we berate them for not taking more interest in what we find so attractive. Yet when we are asked to recommend a book to a non-botanical friend we are sorely puzzled. It is true that we may name Kerner's Natural History of Plants, which no doubt if well read would be greatly edifying, but it costs so much, and is so big a book that few can afford the time or money it demands. We know that it is regarded by many botanists as quite the thing to sneer at Grant Allen's books on plants, but we are not of these, and on the contrary have always admired his ability to state scientific facts—dry facts too—in a way which makes them readable and even entertaining. In his latest booklet—The Story of the Plants—he maintains his reputation for entertaining and at the same time instructive writing. We commend it to the non-botanical who wish to get some general notions of plants, and may we also make bold to suggest that our severely critical and truly scientific botanists run it through. It may be suggestive to them, even.

The author pleasantly tells us "How plants began to be," "How plants came to differ from one another," "How plants eat," "How plants drink," "How plants marry," "Various marriage customs," "The wind as a carrier," "How flowers club together," "What plants do for their young," "The stem and branches," "Some plant-biographies," "The past-histories of plants." That he makes slips here and there may well be granted, but not more, we venture to say, than are made by authors of some more ambitious works.—CHARLES E. BESSEY.

Notes of Botanical Papers.—Edward C. Jeffrey in the December *Annals of Botany* figures and describes polyembryony in *Erythronium americanum,* in which *four embryos* developed in each ovule by the division of the fertilized oosphere.—The freshwater Chlorophyceæ of Northern Russia, are enumerated by O. Borge, in a 40 page paper, accompanied by three plates, the latter mainly of new species or varieties.—The Adirondack Black Spruce is treated fully, both economically and scientifically by Wm. F. Fox the superintendent of state forests for New York, in the Report of the Forest Commission for 1894. This paper has been issued under its title as a separate book of eighty-two pages. It is illustrated by many half-tone and two colored plates. —A. P. Morgan continues his studies of North American Fungi in the

Journal of the Cincinnati Society of Natural History (April–July, 1895) and describes some new genera and species. Three plates accompany the paper, giving illustrations of every species.—In *El Barbareno* (Santa Barbara, California), Mrs. Ida Blochman writes pleasantly and instructively about the California wild flowers. Such papers will do much to help acquaint busy people with the plants about them. It would be well if botanists elsewhere were to imitate Mrs. Blochman.—The recent death of Julien Vesque July 25, 1895) brings to us a series of necrological papers by Dehérain, Bonnier, Duclaux, Schribaux and Bertrand, accompanied by a photogravure of the lamented investigator. Julien Vesque was born in Luxembourg, April 8, 1848, educated in the Grand Ducal Atheneum of Luxembourg, studied in Berlin (under Braun and Kny) and afterwards in Paris with Brongniart, Duchartre and Decaisne. He was early made a member of the Institut Agronomique, in which he was an active worker at the time of his death. The collected titles of his botanical papers number sixty-seven, covering twenty-two years (1873–1895).—Lewis's Leaf-Charts promise to be very useful. They consist of very accurately drawn life-size drawings of characteristic leaves of North American trees. Their moderate price (50 cents per chart, 22 x 28 inches) should warrant their being placed in many of the public schools.—Century III of C. L. Shear's New York Fungi is now in course of distribution. It will prove to be of more than usual interest containing as it does several new or recently described species. This distribution of fungi has met with unusual success, every copy of Century I having long since been taken, no doubt due to the excellence of the specimens. It should be mentioned that the author has removed to Lincoln, Nebr. where he is engaged in botanical studies.

VEGETABLE PHYSIOLOGY.[1]

Change in Structure of Plants due to Feeble Light.—The evidence that new species of plants are developed directly and rapidly out of old ones by changes in the environment is becoming more and more conclusive each year. Plants put into markedly different surroundings either perish or become rapidly modified to meet the changed demands made by the new conditions. One of the most recent and

[1] This department is edited by Erwin F. Smith, Department of Agriculture, Washington, D. C.

interesting pieces of evidence is that brought forward by M. Gaston Bonnier in a long article,—Influence de la lumière électrique continue sur la forme et la structure des plantes,—running through four numbers (78, 79, 80 and 82) of the *Revue générale de Botanique,* Paris, 1895. In a previous study (Les plantes arctiques comparées aux mêmes espèces des Alpes et des Pyrénées, *Rev. générale,* VI, 1894, p. 505) M. Bonnier had shown that arctic plants differ noticeably from the same species growing in alpine regions, e. g., in the greater thickness and simpler structure of the leaves, and had attributed this to the feebler light of the arctic region and to the greater degree of moisture. By means of feeble electric lighting and a moist cool temperature he has now been able to produce these differences synthetically in Paris, i. e., to take alpine plants and convert them into arctic ones. He has also shown by experiments on a great many plants, details of which are given, that feeble continuous electric lighting for a period of six months causes decided histological and morphological changes in nearly all of them, except such as grow in the water. Many plates are given in connection with this paper showing morphology and histology of normally grown and continuously lighted plants and the changes in the structure of the latter are frequently so great that no one would believe the sections to have been derived from the same species. About 75 species were experimented on and these belonged to many different families. The structural changes obtained in *Helleborus niger, Fagus silvatica, Pinus austriaca, Picea excelsa,* and *Pteris tremula* are particularly striking. To illustrate, in the needles of Pinus the characteristic arms or folds of the cortical parenchyma disappear entirely and there are several other equally striking changes. In Pteris the petiolule under the influence of the continuous electric light takes on an epidermis which is clearly distinct from the subjacent cells, and the cells of which are elongated perpendicularly to the surface of the petiole and are much larger than the neighboring layers of cortical tissue while their walls are not thickened; the cell layers immediately under the epidermis (sclerenchymatic tissue) do not become thick-walled and are rich in chlorophyll; the intercellular spaces in the cortical parenchyma have wholly disappeared; and, finally, there is no endodermis, although it is well developed in the normally lighted plant. The palisade tissue was imperfectly formed in bright electric light and in many cases entirely disappeared in feeble electric light thus confirming what has been believed for some time on other grounds, namely that the development of the palisade tissue of leaves stands in direct relation to the intensity of the light. Additional experiments seemed to indicate that most of

the results obtained were due not to the kind of light but to the grade of intensity. The whole paper will repay careful perusal. The author's main conclusions are given in the following paragraphs, as nearly as possible in his own phraseology: *Modifications due to continuous electric light.* The organs completely developed in continuous light have the following characters: (1) The chlorophyll is more abundant and is more uniformly distributed in all the cells which contain it under normal lighting. Moreover, chlorophyll grains may appear even in elements which do not contain them in a normal state, in the bark clear to the endodermis, or even in the medullary rays, in the pith, sometimes even to the central cells of the pith. (2) The structure of the blade of the leaf is simplified; the palisade tissue is less distinct or disappears entirely, the epidermis has cells with thinner walls, and the cortical cells lose their special differentiations (transformation into sclerenchyma of the petioles of the fern, reduplication of the membrane of the cortical cells of the needles of the pine, etc.). (3) The structure of the stem is simplified; the bark is less clearly divided into two different zones or even has all its elements alike; the cork is tardy or but little developed, the endodermis is less well defined, or is no longer distinct from the neighboring cells; the cortical tissue, the tissue of the medullary rays, and that of the pith are composed of elements which more nearly resemble each other; the sclerification and the lignification of the pericycle or of the wood fibres is diminished or disappears entirely; the interior calibre of the vessels is often greater; the perimedullary zone and the libre are less differentiated.

It may be added that the structure in discontinuous electric light approaches more nearly the structure in normal light than that in the continuous electric light. Finally, it should be noted that this latter is intermediate between the normal structure and that in obscurity, except the greening. The simplification of structure under continuous feeble electric lighting is, therefore, to be ascribed partly to the continuity of the light and partly to its feebleness. To sum up, a sort of *green etiolation* is produced by continuous electric lighting, for the two principal characteristics of the changes obtained are the superabundance of chlorophyll and the simplification of the structure.

Somewhat similar results may be obtained by growing plants for some time in weak daylight in the middle of a room and then comparing their structure with that of the same species cultivated in the bright light of a window. Modifications of form and cell structure are still more pronounced if the same plants are grown in total darkness. Anatomical characters are sometimes used in classification and M.

Bonnier suggests that the electric light may be used to determine which of these are most constant.—ERWIN F. SMITH.

A Graft Hybrid.—At a meeting of the Biological Society in Christiana, Nov. 21, 1895, Prof. N. Wille, the well known algologist, exhibited the fruit and leaves of a so-called graft hybrid which is said to have resulted from the working of a pear upon a white thorn (*Cratœgus oxyacantha* L.). This tree stands in the Hofe Torp in Borge Kirchspiel in south east Norway. According to the statement of Herr Apotheker Johns. Smith, of Fredriksstad, the tree is about twenty years old and stood for fifteen years in an unfavorable place without blossoming. It wa then set in a better place and has blossomed and borne fruit for five years. The flowers are like those of the pear tree but somewhat smaller and borne in corymbs like those of *Cratœgus*. The pedicels and the fruit are smooth, but the calyx lobes are triangular and woolly hairy with the tips somewhat bent back. The small fruits (1·5 to 3 cm. long by 1·3 to 2 cm. broad) are pear-shaped but with the color of *Cratœgus* fruits. The fruits are five-celled and usually with two sterile seeds in each compartment; the pericarp is somewhat firmer than the flesh of the fruit and recalls the so-called stone of the *Cratœgus* fruit, but is by no means so hard. The taste of the flesh is insipid and lies between the taste of the pear and that of the white thorn. All the fruits examined by Prof. Wille contained only sterile seeds, but Herr Apotheker Smith stated to him that he once found a single perfect seed. The leaves of the tree have retained the appearance of pear leaves and do not appear to be changed, but out of the wild stem below the point of union shoots of the white thorn now and then grow out and these have the characteristic leaves of that tree. This account is taken from *Biologisches Centralblatt*, Bd. 16, No. 3, Feb. 1, 1896. It would add much to the credibility of this case if it could be learned when, by whom, and from what sort of pear tree this white thorn was grafted. A sceptical pomologist suggests that the top of this tree may possibly be the Japanese *Pirus Toringo*, or some allied species.—ERWIN F. SMITH.

Ustilaginoidea. The following note should have appeared in the March number of this journal, p. 226, after BIOLOGY OF SMUT FUNGI, in connection with which it should be read.

NOTE.—Since the above was written, Dr. Brefeld has succeeded in discovering the full life history of *Ustilaginoidea*. The sclerotia, after lying on damp sand for six months, developed an ascus fructification closely resembling *Claviceps*. Dr. Brefeld's last paper on the subject

may be found in a recent number of *Botanisches Centralblatt*, Bd. 65, No. 4, 1896. It is entitled, Der Reis-Brand und der *Setaria*-Brand, die Entwicklungsgleider neuer Mutterkornpilze.—ERWIN F. SMITH.

ZOOLOGY.

Respiration of Trilobites. Dr. C. E. Beecher comments as follows on the probable method of respiration of the trilobite genus, Triarthrus. " No traces of any special organs for this purpose have been found in this genus, and their former existence is very doubtful, especially in view of the perfection of details preserved in various parts of the animal. The delicacy of the appendages and ventral membrane of trilobites and their rarity of preservation are sufficient demonstration that these portions of the outer integument were of extreme thinness, and therefore perfectly capable of performing the function of respiration. Similar conditions occur in most of the Ostracoda and Copepoda, and also in many of the Cladocera and Cirripedia."

" The fringes on the exopodites in Triarthrus and Trinucleus are made up of narrow, oblique, lamellar elements becoming filiform at the ends. Thus they presented a large surface to the external medium, and partook of the nature of gills." (American Journal Science, April, 1896.)

A Criticism of Mr. Cook's Note on the Sclerites of Spirobolus.—I have read with some interest Mr. Cook's description[1] of certain lines found upon the rings of a specimen of *Spirobolus marginatus*, but I am unable to agree with him in the conclusions drawn from them, nor with his remarks relative to the diplopod segment in general. It seems somewhat surprising that Mr. Cook made no examination of the musculature, either of the specimen described or of any other, to determine whether the lines discovered coincided in any way with lines of muscular attachment, an examination that is necessary to give his conclusions more than a very superficial footing. Had he made the examination, it is extremely doubtful whether he would have found this necessary data, since in more or less closely related forms no lines of attachment corresponding to his lines are to be found.

[1] This journal, p. 333.

Indeed there are many facts that he either ignores or of which he is unaware that are far from lending support to his interpretation of the lines. Some of these I have pointed out elsewhere[2] when considering the subject of the diplopod segmentation. Mr. Cook seems unfortunate in thinking of the greatly overgrown dorsal plate in the diplopod ring as the segment or somite, and in drawing his comparison from the geophilids. Had he examined the conditions occuring in the pauropods and those in *Lithobius*, *Scutigera* and scolopendrids, and taken into account some of the ontogenetic facts known regarding diplopods, he doubtless would have plainly seen indications of alternate plates (not segments) having disappeared and of the remaining plates over-growing the segments behind them, so as to give rise to the anomalous double segments. There would then have been no reason for bringing forward the most decidedly unprogressive supposition, namely, that the double or apparently double condition of the diplopod segment is a condition *sui generis* unexplainable upon general morphological principles.

With reference to his supposition that alternate leg pairs have disappeared even in the geophilids, the case that he has in mind in mentioning the Chilopoda, I must say there is no evidence whatever. To adduce the geophilid condition as evidence is to adduce the thing to be explained. Therefore, I at least am not able to agree with him in saying that this view is no more fantastic than the old fusion idea of Newport, since the latter has some real ground and many favorable appearances in its support, even though it be incorrect.

—F. C. Kenyon.

The Sight of Insects.—M. Felix Plateau has been conducting a series of experiments to settle the question as to whether an insect in flight will go through a net the size of whose meshes would offer no obstruction to the passage of the insect. The question has a bearing upon the difference of vision of Insects and Vertebrates. Mr. Plateau's recent experiments would seem to confirm the statement made by him in 1889 that the vision of insects is obscure as to form, and is adapted more to the perception of movements.

The data upon which the paper is based were acquired by means of ingeniously contrived nettings of various shapes, with meshes 26 to 27 millimeters and 1 to 2 centimeters in size. These nets were placed over attractive lures, such as flowers that insects frequent and in other cases decaying animal matter. The results of the author's observations are given in the following conclusions:

[2] The morphology and classification of the Pauropoda, Tufts College Studies No. 4.

"1. A net extended does not arrest the flight of insects in every case."

"2. During flight the insects act as if they did not see the meshes of the net."

"3. A direct passage by flying is always rare. In the great majority of cases the insect hurls itself upon the net where it rests on one of the threads, and then passes through as any other animal would go through an opening which it discovers."

"4. The only explanation possible for these facts rests on the defective vision due to the compound eyes of Insects. The threads of the net produce in the insect an illusion of a continous surface, just as the cross-hatchings of an engraving do for a human eye. The Arthropod believes itself to be confronted by an obstacle, more or less translucent, in which it can perceive no openings." (Bull. Acad. Roy. Sciences Bruxelles, Nos. 9-10. 1895.)

Dr. Baur on my Drawings of the Skull of Conolophus subcristatus Gray.—In the No. of the Naturalist for April (last p. 238), Dr. Baur criticises Steindachner's drawings of the skull of the above species and my copies of them published in the Naturalist for February, p. 149. He says of the former: "These drawings have not been made to show the detailed relations of the different elements of the skull. Especially the regions copied by Cope are drawn quite insufficiently. The sutures between the different elements can not be made out." To this I have to remark that the sutures between the quadrate and adjacent bones are distinctly drawn, and can be made out perfectly well by any one familiar with the subject, but some of the others are less distinct. Dr. Baur then goes on to say that "Prof. Cope's drawing are not exact tracings from Steindachner for he has drawn sutures which do not exist at all in Steindachner's figure. There is no such suture between the postorbital. Pob, and his supertemporal, St., in the actual specimen, nor in Steindachner's drawing. * * In Prof. Cope's figure the outer and upper portion of the distal end of the paroccipital process separates the parietal process from the prosquamosal (supratemporal Cope.) This is not the case; the parietal process is always united with the prosquamosal. * The prosquamosal (supratemporal Cope) is also drawn quite incorrect; besides, its true relations cannot be made out at all from Steindachner's figures. * *"

It will be noticed that in the above criticism nothing is said about the articulation of the quadr ate with the exoccipital, which is the

question at issue between us; I alleging that the articulation exists, and Dr. Baur denying it. Dr. Steindacher's figures show conclusively that the articulation exists, as it does in nearly all other Lacertilia, and Dr. Baur has not alleged that this plate is wrong in this particular, or that my tracing of it is not an exact copy. On comparing my tracing with the original again, I find that it is an exact copy, and that if any errors exist they are altogether irrelevant to the question at issue. The separation of the parietal process from the superatemporal is shown in Steindachner's plate, but it may be erroneously, as Baur alleges. The suture separating the postorbital from the supratemporal in my drawing may also be an error, but it represents a feature of Steindachner's drawing, which he did not perhaps intend for a suture, although it looks like it. These two points are obscure to the eye without close examination, and it is probable that Baur is right as to their condition in nature. They however do not discredit the accuracy of the conspicous features of the articulations of the elements with the quadrate, which I find to agree with other Iguanidæ.

Dr. Baur's assumption as to what I "really believe," is not quite correct, as can be easily seen by reading my previous articles. What I have endeavored to show is that until the character of the paroccipital (squamosal Baur) of the Pythonomorpha is explained, I hold that the determination of that element as squamosal as is made by Baur, is premature. I am agnostic, and am open to conviction, but Dr. Baur has not yet convinced me.—E. D. Cope.

Zoological News. The Tokio Zoological Magazine, for 1895, Vol. VII contains an account by R. Mitsukuri of a Japanese species of Hariotta, for which he proposes the name *H. pacifica.* The type species of this remarkable chimaeroid genus is now in the U. S. Natl. Mus. It was found in deep water off the coast of Virginia and described by Goode and Bean under the name *H. raleighana.* See Naturalist 1895 p. 375 Plate XIX. The Pacific form agrees with the Atlantic one in general appearance, especially in the elongate muzzle and feeble claspers, but differs in five essential points which are enumerated by the author. The occurrence of this interesting genus in both the Atlantic and Pacific Oceans is an interesting fact.

Recent explorations in the Gulf of California along the coast of Sinaloa have resulted in a collection of fishes, which while yielding 232 species, by no means exhausts the richness of that locality. The collection was sent to Prof. Jordan for identification. Thirty new species were found among the specimens, all of which are described

and figured in the proceedings of the Calif. Acad. Sci. Vol. V. 1895.

Among the new fishes described during the past year is *Razania makua* from the Hawaiian Islands. The species is very rare, only two specimens being known. It is a deep-sea fish by habit, and is especially remarkable for its rapidity in swimming. A colored plate accompanies the description given by Mr. O. P. Jenkins in the Proceeds. Calif. Acad. Sci. Vol. V., 1895.

Two new genera and species of fishes, belonging to the family Percophidæ, are reported from Australia, by J. D. Ogilby. They are described by him under the names *Centropercis nudivittis* and *Tropidostethus rothophilus.* The latter are surf-fishes, never descending to the bottom, but swimming a few inches beneath the surface of the water. (Proceeds. Linn. Soc. N. S. W. (2) Vol. X. Pt. 2, 1895.)

In an examination of 52 specimens of *Vipera berus* from Denmark, Mr. Boulenger finds a wide range of individual variation. The differences observed are in the shape of the snout, the scaling of the head, body and tail, size and coloration. The observations as to color confirm those previously made by Geithe in Germany. (Zoologist, 1895.)

The same author in a recent classification of the American Box Tortoises in the British Museum, adopts Baur's definitions of species and distinguishes six of which he gives a synopsis. He holds to the generic name of Cistudo although it has been shown that Terrapene has priority. Ann. Mag. Nat. Hist. 1895.)

ENTOMOLOGY.[1]

A New Diplopod Fauna in Liberia.—From the west coast of Africa large numbers of Diplopoda are already known, and yet very little of the vast extent of territory has been thoroughly searched for members of this group. In connection with an attempted exploration of Liberia under the auspicies of the New York State Colonization Society, there has been an opportunity for careful collecting in the western part of that country, some of the results of which are here offered. The majority of Liberian Diplopoda belong to the suborder Polydesmoidea. The only other families represented are the Polyxenidæ, Stemmatoiulidæ, Spirostreptidæ and Spirobolidæ, and these offer no very remarkable novelty in structure or form. This is in strong contrast to the great number and variety of Polydesmoidea; indeed it

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

has proved necessary to establish genera and families in attempting to properly recognize their structural novelty and diversity. Some of these new groups have already received names,[2] but their characters have been only formally indicated.

Family Ammodesmidæ.

Two minute Glomeroid genera were discovered, one of which, Ammodesmus, is the smallest member of the suberder, if not of the entire class. The only species, *Ammodesmus granum*, is less than two millimetres long, and about half a millimetre broad. A single specimen was secured while collecting minute Oniscidæ, but diligent and repeated search failed to find another. It did result, however, in three specimens of a very distinct, though evidently allied, genus which it is proposed to name Cenchrodesmus. Both genera have the habit of coiling into a sphere. The second segment is enormously enlarged so as to completely conceal the head and first segment when viewed from the side, as well as to cover the space left between the decurved carinæ of the other segments when the creatures are coiled. Ammodesmus has the dorsum roughened by a transverse row of large papilliform tubercles rising from the posterior part of each segment, while *Cenchrodesmus volutus* has the segments nearly smooth. The surface of Ammodesmus is rough and dusted with earth. When disturbed it coils up and lies motionless, and is then perfectly concealed, having exactly the appearance of a grain of sand. My specimen would certainly not have been seen had it not been crawling. Cenchrodesmus is pinkish in life and mottled with pale horn-color in alcohol. Both genera live on the ground under decaying wood or leaves.

Family Campodesmidæ.

This also contains two genera similar in size and general shape, yet evidently distinct, in that Campodesmus has the segments ornamented with two conspicuous clusters of coarse tubercles, while *Tropidesmus jugosus* has two transverse rows of short longitudinal carinæ, a form of sculpture previously quite unknown in Polydesmoidea. The carinæ are depressed in both genera, and the dorsal surface is very rough with fine granules and tubercles. Pores are visible on the fifth and seventh segments, but I have been unable to find them on the others. Both forms are denizens of the deepest forests, where the light is so deficient that they are sure to be overlooked unless specially sought for. They are very sluggish in their movements, and are seldom found crawling. When disturbed they coil up into a spiral, and also assume that posi-

[2] Proc. U. S. Nat. Museum and Annals N. Y. Acad. Science, 1895.

tion in alcohol. The first segment is not enlarged to conceal the head, nor are the anterior segments larger than the others. The general appearance is strikingly different from that of other Diplopoda, the resemblance being rather to certain lepidopterous larvæ.

Familly Comodesmidæ, new.

The type of this family is a small, reddish-brown, subcylindrical form, very rare, and also inhabiting the denser parts of the forest. The pore-formula is unique: 5, 7, 9, 12, 15, 17, 18. The pores are located in the front part of the posterior subsegments. The dorsal surface is beset with conic piliferous granules, giving a wooly appearance. The last segment is scarcely produced beyond the anal valves, but is rounded off at apex as in many Iulidæ. The head is not concealed by the first segment, which is narrower than the second and somewhat included between the carinæ of the latter, much as in *Scytonotus granulatus* (Say). Two other allied genera, also granular and hairy, are found in similar situations in Liberia, but both have the normal pore-formula as in Polydesmus. Thelydesmus is nearly black, larger and much more abundant than Comodesmus. The generic name alludes to the fact that the females are in a large preponderance. Although about a hundred females were taken, careful and extended collecting resulted in only four males. The remaining genus is minute and very rare, cylindrical, and without carinæ. The posterior subsegments are abruptly thicker than the anterior, giving the appearance of a series of of discs laid together, whence the generic name, Discodesmus. In the Berlin Museum is another form evidently allied to Thelydesmus, but with broader carinæ and more resemblance to the Pterodesmidæ, to be noted later on. It was collected in the German Colony of Togo by Dr. K. Büttner, and may be known as *Xyodesmus planus*.

In addition to the above genera there may be referred to this family Cylindrodesmus Pocock, from Christmas Island. It is even more evidently allied to Comodesmus than the other genera mentioned. There is also in my collection a new generic type from the mountains of Java, not closely related to the other genera, but evidently belonging to the same family group.

Family Prepodesmidæ, new.

Under this name it is proposed to arrange West African forms hitherto referred either to Paradesmus or to Oxydesmus, from the latter of which they differ in having the apex of the last segment narrow and bituberculate. The affinities of the group seem to be with the Oxydesmidæ, although no connecting links have yet turned up. In a

large number of forms the poriferous segments are wholly or partly red or yellow, while the remainder of the body is nearly black, giving a most striking appearance. Prepodesmus includes several such forms, all with the anterior corner of the second segment greatly produced and embracing the first segment. Tylodesmus has the corner rounded and not produced. Cheirodesmus is similar to the last in general shape, but is more slender and with the male genitalia resembling in shape a gloved hand. Anisodesmus is peculiar in that the fourth segment is distinctly, though slightly, narrower than the third or fifth. The species are uniform dark red in color and the type is closely allied to *Polydesmus erythropus* Lucas. Isodesmus is evidently related, but with the fourth segment not narrowed, and remarkable in that the pores are not borne on a distinct callus as in the other genera of the group. The copulatory legs are also very peculiar being deeply divided into several laciniæ. In all these genera the dorsal surface is finely and evenly granular, though differing somewhat in other respects. The family is probably distributed along the entire West Coast, and I have seen two forms from South Africa, one of which, Lipodesmus, is in the Berlin Museum.

Family Oxydesmidæ.

The Liberian forms which belong to this family in the more limited sense[3] are all referable to the genus Oxydesmus, and belong to three species, *O. grayii* Newport, *O. medius* and *O. liber*, both new. The first is a very striking form, black in color with a narrow median stripe of bright vermillion. The other species are also black, *O. liber* with bright yellow submarginal ridges.

Family Polydesmidæ.

Of Liberian species referable to this family in its stricter sense there seem to be but two; small pinkish-red forms, similar in general appearance to some species of Brachydesmus. The dorsal elevated areas are each supplied with a clavate hair. The antennæ are strongly clavate, though rather slender, and the second pair of legs is crassate in the

[3] The African forms having the apex of the last segment broad, the femora spined, and the carinæ with a submarginal ridge, constitute the family Oxydesmidæ. There are five genera now known, two confined to the east side of the continent, three to the west. Of the east coast forms, Orodesmus includes those with strongly tuberculate segments, Mimodesmus those with the body slender and the dorsum nearly smooth. Of the west coast genera Oxydesmus has three rows of dorsal tubercles and surrounding areas; Scytodesmus has five or six rows, while Plagiodesmus resembles Oxydesmus, but has the submarginal ridges very broad and oblique, and the copulatory legs large and exposed.

male. For this genus the name Bactrodesmus is proposed; it will probably be found to be next related to the form described from Ceylon by Humbert as *Polydesmus cognatus*, but which is generically different from the European *P. complanatus*, and may be denominated Nasodesmus.

Family PTERODESMIDÆ, New.

This family is proposed for *Polydesmus gabonicus* Lucas and its African relatives, by more recent writers referred to Cryptodesmus. I have examined the type of *Cryptodesmus olfersii* in the Berlin Museum. The diversities seem to be of family importance. The African forms are very curious, the development of the lateral carinæ being carried to its greatest extent. They are very much flattened, elliptical in outline, and only four or five times as long as broad. They never coil into a spiral, even when placed in alcohol. At least five genera are found in Liberia.

All the African forms yet known to me have repugnatorial pores, and we may expect to find these in the others, notwithstanding the statements of several writers to the contrary. The location of the pores is, however, very unusual. They are far remote from the lateral margin, in the *anterior* part of the carinæ, in some cases so far ahead as to be concealed by the posterior margin of the preceeding segment. An even more remarkable condition obtains in *Pterodesmus brownellii*, the type of the genus and family. The fifth segment has no pore! The Liberian forms are further peculiar in that all are more or less pruinose. Pterodesmus is the largest of the Liberian genera. It is pure white when young, but mature individuals are usually dusted with earth which adheres to the pruinosity and gives them the advantage of protective coloration. Gypsodesmus, on the other hand, is pure white, even when mature. Lampodesmus is partly pruinose and appears to be black and white when alive, though it is brown in alcohol. It is structurally peculiar in that the sternum of the sixth segment bears two hollow processes fringed along their apical edges with long hairs. These may be of use as a protection to the copulatory legs. Compsodesmus is the broadest of the Liberian forms. When alive it is one of the most varied and brilliant of Diplopoda. A large median area of the dorsal surface of each segment is dark brown, while the space between it and the posterior margin on each side is nearly white or bright yellow. Carinæ tinted with bright orange or pink, or both. Below, except near the edges of the carinæ, the body is covered with a pure white bloom or chalky powder. Last segment nearly white. Motions very sluggish.

From the German colony of Togo comes a genus evidently allied to the last, but distinct by reason of the more slender body and narrower carinæ, which are also scarcely produced at the posterior corners. From Lampodesmus it is distinct in the absence of the process from the sternum of the sixth segment, and in the form of the copulatory legs.

A small horn-brown or yellowish creature with remarkably agile movements it is proposed to name *Choridesmus citus.* The first segment is pure white, pruinose, and abruptly different in color from the remainder of the body. The pores are large, and are located in the middle of the carinæ, remote from the margins. The quick, jerky movements remind one strongly of Polyxenus.

Family STRONGYLOSOMATIDÆ.

Of this group there are two genera in Liberia, both new, though probably not confined to the West Coast. *Scolodesmus grallator* represents the usual Strongylosoma type, with long legs and antennæ. It is dark wine-color, nearly black. *Habrodesmus lætus* is a rare species apparently confined to the darkest forests. It is exceedingly quick and agile, very graceful in form and brilliantly colored. The legs are orange and pink, and the segments have the posterior margin yellow, shading through orange and brown to black on the remainder of the segment.

In gardens at Monrovia *Orthomorpha vicaria* (Karsch) is not uncommon; it is probably not indigenous.

Family STYLODESMIDÆ.

The type of this family is a bizzare creature named *Stylodesmus horridus.* The generic name alludes to the fact that the pores are borne on long stalks placed near the lateral margins of the broad, decurved carinæ. The pore-formula is the usual one, 5, 7, 9, 10, 12, 13, 15–19. The whole dorsal surface of the animal is setose and coal-black. There is almost always an incrustation of dirt which furnishes a completely protective coloration. The head is completely concealed under the flabelliform, anteriorly lobed, first segment, and the last segment is reduced, included in, and concealed by the penultimate. The most striking feature is that each of the segments except the last bears dorsally a pair of long slender processes. Those of the anterior and posterior segments are close together and show a tendency to unite at the base. These processes are also rough and setose, and almost always so incrusted with dirt as to appear several times their actual size. If segments of Stylodesmus had been found in fossil condition they would probably have been looked upon as allied to some of the

Archipolypoda, so much greater is the general resemblance to the fossils than to previously known extant genera. Yet there are in Liberia at least three other genera which have evident affinities with Stylodesmus. In all the pores are located on special processes or tubercles, and the first segment is enlarged and scalloped in front. *Udodesmus telluster* differs from Stylodesmus in being much more slender and without the long dorsal processes. These are replaced by two longitudinal crests of two or three large tubercles. The body is very rough, setose, and incrusted with earth. *Hercodesmus aureus* is a beautiful little species, more slender than Udodesmus, and usually without a covering of earth. In Stiodesmus the dorsal ridges of tubercles are not much more prominent than the numerous large, rounded tubercles with which the whole surface is beset. The result is a creature which on first view might be supposed to have affinities with Scytonotus.

Besides these, the present family will contain four East Indian genera, Pyrgodesmus and Lophodesmus, described by Pocock, and two new ones from Java. In the Canary Islands is a beautiful and evidently allied form inhabiting the nests of ants, and called Cynedesmus, on account of the form of the first segment. The Stylodesmidæ do not coil up into a close spiral; they usually remain nearly straight, even when in alcohol. Though there is no close resemblance in form or structure between the Stylodesmidæ and Campodesmidæ, yet both are strikingly different from other Diplopoda. That two groups of such remarkable creatures should inhabit the same locality seemed at first very strange, but as the various new and equally interesting forms continued to be found it was soon apparent that we were really in the presence of a new fauna.

That the new families are not all confined to Africa is shown by recent papers, notably those of Mr. Pocock. As yet, however, the Ammodesmidæ and Campodesmidæ are known only from African representatives. Of the larger, long known forms the Oxydesmidæ and Prepodesmidæ, appear to be confined to Africa. In East Africa is another family of several genera, none of which has yet been reported from the West Coast. Indeed, speaking with regard only to families and genera, there are four very distinct diplopod faunæ in the African continent, the northern, southern, eastern and western parts having little in common. The species are, of course, even more local. I have examined the collections of the Berlin Museum and the British Museum, as well as the literature of the subject, and with the exception of *Oxydesmus grayii* and *Orthomorpha vicaria*, collected at Sierra Leone, know of no Liberian diplopod from any other part of the West Coast.

We are thus assured of an African fauna of surpassing richness, not a tithe of which has yet been revealed.—O. F. Cook.

Entomological News.—Prof. Clarence M. Weed of the New Hampshire College spent several weeks in December and January, studying the Bermuda Islands. Many species not before recorded from there were collected.

EMBRYOLOGY.[1]

The Sense Plates, the Germ of the Foot, and the Shell or Mantle Region in the Stylommatophora.[2]—To our knowledge of these subjects, Dr. Ferdinand Schmidt contributes the results of his numerous observations upon the embryos of *Succinea*, *Limax* and *Clausilia*. Concerning the sensory plates he shows that immediately behind the budlike rudiments of the future egg-bearing and the simple tactile tentacles, in *Limax* where the development is most easily followed, there arises a third pair of buds like the first two pairs in all respects except in size. From these buds arises the so called oral lobes, subtentacular lobes, or labial tentacles. They have no relation to the velum whatever, since they arise in a pre-velar region. This is completely at varience with the observations of Jayeux-Laffuie on *Onchidium* and those of Ray Lankester on *Limnæus* in which the subtentacular lobes are asserted to arise from the velum or a rudiment of the same. Should further studies upon these forms substantiate the assertion, we would then have two groups of oral lobes, one in which they arise from the velum and to be homologized with the oral lobes of the lammellibranchiata, where they undoubtedly have such an origin, and the other in which they arise from the sensory plates and are homologous with the tentacles.

In his account of the development of the foot in *Succinea* he supports the conclusion long since put forth by Lankester, namely, that the typical form of the blastopore is an elongated cleft on the ventral side of the embryo, from which arises in some cases the mouth, in others the anus, according as the cleft persists anteriorly or posteriorly. This form of a blastopore is certainly important, considering his con-

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts reviews and preliminary notes may be sent.

[2] Beitrage zur Kenntniss der Entwicklungsgeschichte der Stylomatophoren, with 9 text figs. Zool. Jahrbücher, VIII, 318.

clusions with regard to the foot. This, he says, is to be distinguished very much earlier than has hitherto been recorded and, as one would naturally expect from Patten's study of *Patella*, which he quotes, it arises from a pair of folds and not from a single one as has generally been stated for related forms. In *Succinea* these two folds appear close behind the blastopore between the region of the mouth and anus, and approaching one another fuse in the median line forming an oval area. A median furrow persists for some time as evidence of the union as in *Patella*. This last fact gives some meaning to the similarly furrowed appearance occuring in *Limnœus, Planorbis* and *Ancylus*.

A study of over 100 embryo showed him that this paired origin is the rule, although examples were found where the elevation was unpaired, forming then a broad disc. In one apparently pathological case the blastopore had retained its supposed primitive elongated form and the beginning of the foot had the form of a horseshoe embracing its hinder end.

His conclusion that the foot represents the fused lips of the elongated blastopore removes the possibility of the organ being some kind of secondary formation, and makes it out to be a metamorphosed very ancient structure: and if the conclusion is correct, the molluscan foot is not quite such an anomalous structure as it has hitherto seemed.

A few remarks concerning the podocyst and the so called "Nackenblase" are of interest in that they show that the latter structure is not an organ at all, and that the contracting motions that have been observed in it are due to the contractions of the podocyst which acts as an organ of circulation. For in *Succinea* where the structure in question has an enormous development and where no podocyst occurs there are no such movements to be seen. The structure is, he says, a mass of endoderm cells swollen with albumen, the embryonal liver and the outer body epithelium.

With regard to the shell gland, Schmidt substantiates, in the main, the early observations of Gegenbaur or *Clausilia* and shows Korschelt's doubt concerning them to be unfounded. A large series of *Clausilia* embryos gave ample opportunities for study, and as a result it appears that very early the shell gland arises as an invagination of the outer epithelium, and closing up, becomes completely cut off from its parent layer. Sections show it to be completely surrounded by mesoderm. The hollow vesicle thus formed becomes flattened out so that he distinguishes in it an outer and an inner layer of cells separated by a narrow space. The outer layer remains more or less un-

changed, but the cells of the inner one proliferate and begin to lay down the shell, which may be distinguished in sections as a very thin lamina. At about this time observations of embryos by reflected light show a small invagination or hole near the center of the newly formed shell, which is thus laid bare. The hole then is of secondary formation and not, as Korschelt supposes, something that has persisted from the original invagination.

It appears then that the internal formation of the shell, as it has been generally recognized in the so called naked pulmonates is not an exception to a rule but the rule itself, and that the condition obtaining in *Limax* and others differs from that in the rest of the pulmonates only in so far as a rudimentary condition is retained in the adult animal.—F. C. KENYON, Ph. D., Clark University, Worcester, Mass.

PSYCHOLOGY.

Physical and Social Heredity.—The great courtesy of the Editor of this journal in reprinting one of my paper from *Science* preliminary to replying to it encourages me to ask him for a page or two of comment on his reply. This is the more needful since the second of my papers which he criticises may not have been seen by the readers of the NATURALIST, and the third has only just appeared in *Science*, (March 20 and April 10, 1896).

The main question at issue is the relation of consciousness or intelligence to heredity; the other matter, that of the relation of consciousness to the brain, being so purely speculative that I shall merely touch upon it at the end of this note.

Prof. Cope[1] says: " there is no way short of supernatural revelation by which mental education can be accomplished other than by contact with the environment through sense-impressions, and by transmission of the results to subsequent generations. The injection of consciousness into the process does not alter the case, but adds a factor which necessitates the progressive character of evolution." Both of these sentences I fully accept, except that the word " transmission " seem to imply the Lamarkian factor, which I think the presence of consciousness renders unnecessary. Using the more neutral word " conservation " instead of " transmission," I may refer to three points on which Prof. Cope criticises my views: first, conservation of intelligent acquisitions from genera-

[1] AMER. NAT., April, 1896, p. 343.

tion to generation; second, "the progressive character of evolution;" and third, "mental education" or acquisition.

First, agreeing as we do on the fact of mental acquisition or "selection through pleasure, pain, experience, association, etc.," Prof. Cope cites my second paper (*Science*, Mar. 20th) in which I hold that consciousness makes acquisitions of new movements by such selections. He then says, if so, then I admit the Lamarkian factor. But not at all; it is just the point of my article to refute Romanes by showing that adaptation by intelligent selection makes the Lamarkian factor unnecessary. And in this way, i. e., this sort of adaptation on the part of a creature *keeps that creature alive* by supplementing his reflex and instinctive actions, so *prevents the operation of natural selection* in his case, and gives the species time to get congenital variations in the lines that have thus proved to be useful (see cases cited). Farthermore, all the resources of Social Heredity—the handing down of intelligent acquisition by maternal instruction, imitation, gregarious life, etc.—come in directly to take the place of the physical inheritance of such adaptations. This influence Prof. Cope, I am glad to see, admits; although in admitting it, he does not seem to see that he is practically throwing away the Lamarkian factor. For instead of limiting this influence to human progress, we have to extend it to all animals with gregarious and family life, to all creatures that have any ability to imitate, and finally to all animals which have consciousness sufficient to enable then to make conscious adaptations themselves: for such creatures will have children that do the same, and it is unnecessary to say that the children must inherit what their fathers did by intelligence, when they can do the same things by their own intelligence. As a matter of fact Prof. Cope is exactly the biologist to whose Lamarkism this admission is, so far as I can see, absolutely fatal; for he more than all others holds that adaptations all through the biological scale are secured by consciousness.[2] If so, then he is just the man who is obliged to extend to the utmost the possibility of the transmission also of these adaptations by intelligence, which, as I said, rules out the need of their transmission by physical heredity. At any rate he is quite incorrect in saying that "he [I] both admit and deny Lamarkism."

To this argument of mine Prof. Cope presents no objection that I see except one from analogy. He says: "I do not see how promiscuous variation and natural selection alone can result in progressive psychic evolution, more than in structural evolution, since the former is condi-

[2] And in this I think he is right: see chaps. VII and IX of my *Mental Development* (Macmillans, 2d. ed.).

tioned by the latter." As to the word "progressive," I take up that question below; but as to the analogy with structural evolution, two answers occur to me. In the first place, Prof. Cope is, as I said, the very man who holds that all structural evolution is secured by direct conscious adaptations. He says: "mind determines movements and movements have determined structure or form." If this be true how can psychic be conditioned by structural evolution? Would not rather the structural changes depend upon the psychic ability of the creature to effect adaptations? And then, second, at this point Prof. Cope assumes the Lamarkian factor in structural evolution. Later on he makes the same assumption when he says: "But since the biologists have generally repudiated Weismannism," etc. This is a curious saying; for my impression is that even on the purely biological side, the tendency is the other way. Lloyd Morgan has pretty well come over; Romanes took back before he died many of his arguments in favor of the Lamarkian factor; and here comes a paleontologist, Prof. Osborn, —if he is correctly reported in *Science*, April 3rd, p. 530—to argue against Prof. Cope on this very point with very much the same sort of argument as this which I have made.[3] And while Prof. Cope will agree with me that this sort of *argumentum ex autoritate* is not very convincing, yet he will not object to my balancing off his dictum with the following from a letter which just comes to me from another distinguished biologist, Prof. Minot: "Neo-Lamarkism seems to me an impossible theory."

But Prof. Cope goes on to say that I "both admit and deny Weismannism;" on the ground that "his [my] denial of inheritance only covers

[3] Since writing this I have heard Prof. Osborn read a paper which confirms the agreement between him and me which I supported in the text above. I reached my conclusion independently and one of my *Science* articles gives report of it as expressed in a criticism of Romanes before the New York Academy of Science on Jan. 31st, 1896. Prof. Osborn's expression "ontogenic variations" i. e. those brought out by "environment (which includes all the atmospheric, chemical, nutritive, motor, and psychical circumstances under which the animal is reared)" seems to make these adaptations after all *constitutional*. As Prof. Osborn says this will not do for all cases; and I think it will not do for instinct, where constitutional variations without the aid of *intelligence* would never suffice (as Romanes says) to keep the animal alive while correlated variations are being perfected phylogenically. But it seems to answer perfectly where intelligent or other adaptations supplement the constitutional variations—and that is just the point made in my *Science* paper. As to the way these ontogenic variations or adaptations are brought about in the individual creature, see the remarks on "organic selection" below. I am printing in the next issue of this journal (June) a full statement of the entire position.

the case of psychological sports." But I do not see the connection. If Prof. Cope means denial of the inheritance of acquired characters then I deny it equally of sports and other creatures; but I do not deny that the native "sportiveness" (!) of sports tends to be transmitted. In my view the "massiveness of front" which progress shows (and which Prof. Cope accepts) shows that in social transmission the individual is usually swamped in the general movement as the individual sport is in biological progress. As a matter of fact, however, the analogy from "sports" which Prof. Cope makes does not strictly hold. For the social sport, the genius, is *sometimes* just the controlling factor in social evolution. And this is another proof that the means of transmission of intelligent adaptations is not physical heredity alone, but that they are socially handed down. I do not see, therefore, what Prof. Cope means by saying that I "admit and deny Weismannism," for I have never discussed Weismannism at all. I believe in the Neo-Darwinian position plus some way of getting "determinate variations." And for this latter I think the way now suggested is better than the Lamarkian way. Like many of the biologists (e. g., Minot) I see no proof of Weismannism (just as I protest mildly against being sorted with Mr. Benjamin Kidd!); yet I have no competence for such purely biological speculations as those which deal in plasms!

Second, the question as to how evolution can be made "progressive." Prof. Cope thinks only by the theory of "lapsed intelligence" or "inherited habit." Admitting that the intelligence makes selections, then they must be inherited, in order that the progress of evolution may set the way the intelligence selects. But suppose we admit intelligent selection (even in the way Prof. Cope believes); still there are two influences at work to keep the direction which the intelligence selects apart from the supposed direct inheritance. There is that of social handing down, imitation, etc., or Social Heredity, which I have already pointed out; and besides there is the survival by natural selection of those creatures which have variations which intelligence can use. This puts a premium on these variations and their intelligent use in following generations. Suppose, for instance, a set of young animals some of which have variations which intelligence can use for a particular adaptation, thus keeping these individuals alive, while the others who have not these variations die off; then the next generation will not only have the same variations which intelligence can use in the same way, but will also have the intelligence to use the variations in the same way, and the result will be about *the same as if the second generation had inherited the adaptations directly.* The direc-

tion of the intelligent selection will be preserved in just the same sense. I think it is a great feature of Prof. Cope's theory that he emphasizes the intelligent direction of evolution, and especially that he does it by appealing to the intelligent adaptations of the creatures themselves; but just by so doing he destroys the need of the Lamarkian factor. Natural selection kills off all the creatures which have not the intelligence nor the variations which the intelligence can use; those are kept alive which have both the intelligence and the variations. They use their intelligence just as their fathers did, and besides get new intelligent adaptations, thus aiding progress again by intelligent selection. What more is needed for progressive evolution?[4]

Third. We come now to the third point—the method of intelligent selection—and on this point Prof. Cope does not understand my position, I think. I differ from him both in the psychology of voluntary adaptations of movement, and in the view that consciousness is a sort of force directing brain energies in one way or another (for nothing short of a force could release or direct brain energy). The principle of Dynamogenesis was cited in my article in this form: i. e., "the thought of a movement tends to discharge motor energy into the channels as near as may be to those necessary for that movement." This principle covers two facts. First, that no movement can be thought of effectively which has not itself been performed before and left traces of some sort in memory. These traces must come up in mind when its performance is again intended. And second (and in consequence of this) that no act, whatever, can be performed by consciousness by willing movements which have never been performed before. It follows that we can not say that consciousness by selecting new adaptations beforehand can make the muscles perform them. The most that psychologists (to my knowledge) are inclined to claim is that by the attention one or other of alternative movements which have been performed before (or combinations of them) may be performed again; in other words, the selection is of old alternative movements. But this is not what Prof. Cope seems to mean; nor what his theory requires. His theory requires the acquisition of new movements, *new adaptations to environment*, by a conscious selection of certain movements which are *then carried out the first time* by the muscles.[5]

[4] I keep to "intellegent" adaptations here; but the same principle applies to *all adaptations made in ontogenesis.* I am using the phrase "Organic Selection" in the article to appear in this journal to designate this "factor" in evolution (see the next heading below).

[5] "Conscious states do have a causal relation to the other organic processes." I do not find, however, that Prof. Cope has made clear just how in his opinion the "selection" by consciousness does work.

It may very justly be asked; if his view be not true, how then can new movements which are adaptive ever be learned at all? This is one of the most important questions, in my view, both for biologists and for psychologists; and my recent work on *Mental Development* is, in its theoretical portion (chap., VIIff), devoted mainly to it, i. e., the problem of *ontogenetic accommodation.* I can not go into details here, but it may suffice to say that Spencer (and Bain after him) laid out what seems to me, with certain modifications urged in my book, the only theory which can stand in court. Its main thought is this, that all new movements which are adaptive or "fit" are selected from *overproduced movements* or *movement variations*, just as creatures are selected from overproduced variations by the natural selection of those which are fit. This process, as I conceive it, I have called "organic selection," a phrase which emphasizes the fact that it is the organism which selects from all its overproduced movements those which are adaptive and beneficial. The part which the intelligence plays is "through pleasure, pain, experience, association, etc., to concentrate the energies of movement upon the limb or system of muscles to be used and to hold the adaptive movement, "select" it, when it has once been struck. In the higher forms both the concentration and the selection are felt as acts of attention.

Such a view extends the application of the general principle of selection through fitness to the *activities of the organism.* To this problem I have devoted some five years of study and experiment with children, etc., and I am now convinced that this "organic selection" bears much the same relation to the doctrine of special creation of ontogenetic adaptations by consciousness which Prof. Cope is reviving, that the Darwinian theory of natural selection bears to the special creation theory of the phylogenetic adaptations of species. The facts which Spencer called "heightened discharge" are capable of formulation of the principle of "motor excess": "the accommodation of an organism to a new stimulation is secured—not by the selection of this stimulation beforehand (nor of the necessary movements)—but by the reinstatement of it by a discharge of the energies of the organism, concentrated, as far as may be, for the excessive stimulation of the organs (muscles, etc.), most nearly fitted by former habit to get this stimulation again,"[6] in which the word "stimulation" stands for the condition favorable to adaptation. After several trials with grotesquely excessive movements, the child (for example) gets the adaptation aimed at more and more perfectly, and the accompanying excessive and use-

[6] *Mental Development*, p. 179. Spencer and Bain hold that the selection is of purely chance adaptations among spontaneous random movements.

less movements fall away. This is the kind of "selection" that consciousness does in its acquisition of new movements. And how the results of it are conserved from generation to generation, without the Lamarkian factor, has been spoken of above.

Finally, a word merely of the relation of consciousness to the energies of the brain. It is clear that this doctrine of selection as applied to muscular movement does away with all necessity for holding that consciousness even directs brain energy. The need of such direction seems to me to be as artificial as Darwin's principle showed the need of special creation to be for the teleological adaptations of the different species. This necessity of supposed directive agency done away in this case as in that, the question of the relation of consciousness to the brain becomes a metaphysical one; just as that of teleology in nature became a metaphysical one; and science can get along without asking it. And biological as well as psychological science should be glad that it is so—should it not?

I may add in closing that of the three headings of this note only the last (third) is based on matters of my private opinion; the other two rest on Prof. Cope's own presuppositions—that of intelligent selection in his sense of the term, and that of the bearing of Social Heredity (which he admits) upon Lamarckism. In another place I hope to take up the psychology of Prof. Cope's new book in some detail.

J. Mark Baldwin.

Observations on Prof. Baldwin's Reply.—In order to comprehend the question at issue, it is necessary to state certain fundamental principles of evolution. This process consists in the development of the heterogeneous from the homogeneous as Spencer expresses it; or in more specific language, evolution consists in the development of specialized structures from generalized material. Primitive organic or living beings consist of protoplasm which is, as compared with higher organisms, generalized. That is, they are without distinct muscular, nervous, or digestive organs, etc. How are psychic conditions related to this process of specialization? Prof. Baldwin states that an animal is able to "select through pleasure, pain, experience, association, etc., from certain alternative complex movements which are already possible for the limb or member used." This means that under guidance of a form of consciousness, certain existing muscles are selected to perform certain movements, while other muscles are neglected. Now if this be possible to a muscular system specialized into discrete bundles, it is also possible to a primitive contractile protoplasm which is not yet differ-

entiated into discrete muscular and other bodies. In other words it is possible to contract that part of the homogeneous protoplasm which is necessary for the production of a certain movement, and leave that part of the protoplasm which is not necessary to produce the movement, uncontracted. And this is exactly what undifferentiated animals (Protozoa) do, and it is what is done at all stages of differentiation of the muscular system, so far as the differentiation which that muscular system has attained, will permit. It is the sentence which I have quoted above from Prof. Baldwin which induced me to say that he admits the Lamarkian factor. For there is no doubt that it has been this habitual contraction of certain parts of undifferentiated protoplasm which has produced muscular bands, sheets, etc., as distinguished from other histological elements of the organism. If this be true, there is no necessity for the hypothesis of "overproduced movements" as the source of new habits, since those habits may be produced by the direct effect of the selective power of the animal over its own protoplasm. It is not intended by this expression to claim anything more than simple sensation for simple forms of life, or that anything higher than hunger, reproduction temperature, etc., constitute their pleasures and pains.

The theory of natural selection from "overproduced movements" as a *source* of new movements stands on the same basis as all the other theories of natural selection as explanations of the *origin* of anything new. They are impossible in practice, and inaccurate in logic, since in my opinion, following that of Mr. Darwin, they demand of Natural Selection a function of which it is by its definition incapable. That natural selection regulates the survival of movements after they have originated, goes without saying. It is evident that "overproduced movements" must on Prof. Baldwin's "Organic Selection" theory, include the adaptive one which is destined to survive. The question then is as to the origin of this particular "overproduced" and adaptive movement. The explanation has been given above; i. e. that it is a direct response to the stimulus supplied. The location in the organism of the responsive movement depends on the location of the stimulus, a fact testified to by the close local connection of motor with sensory nerves of general sensation. In the case of responses to special sensation, we may suppose that the responses only became exact as to locality after a period of trial and error, the new movement always having a local relation to the point of stimulus. The beast bites his wound, before he has traced the pain to his enemy. As already pointed out, this process would result in a perfected mechanism which would be inherited. No one can yet explain the mechanism of the control of a mental state

over a contraction of protoplasm. It is one of the ultimate facts of the universe. When Prof. Baldwin admits that an animal can select which of two muscles it will use, or when he admits that an animal can contract any muscle under the stimulus of "pleasure, pain, etc., he admits this ultimate fact, but does not explain it.

As to the scope of Social Heredity as a factor in psychic evolution, it appears to me to be, like that of the higher intelligence, mainly restricted to the higher animals and to man. Maternal instruction among all but the higher animals probably has no existence. Imitation may be supposed to be possible to animals a little lower in the scale. But both factors are to my mind only supplementary to the more vigorous education furnished by the environment, with its wealth of stimuli to "pleasure, pain, experience, association, etc." In regarding Social Heredity as the sole factor of psychic evolution, Prof. Baldwin temporarily loses sight of the intimate connection between mind and its physical basis. The inheritance of mental characteristics is as much a fact as the inheritance of physical structure, and for the reason that the two propositions are identical. One does not believe in either education or imitation as a cause of the repetition of insanity in family lines. We rather believe in a defective brain mechanism, which is inheritable, though fortunately not always inherited. The doctrine of Weismann that acquired characters are not inherited, if true, would furnish the physical conditions for the theory that Social Heredity is the only psychic heredity, but it is impossible to believe that Weismann's doctrine is true. Hence while Social Heredity is true as far as it goes, Lamarkism is also true, and expresses the more fundamental law. The fact that no adequate physical explanation of the inheritance of acquired characters has been reached does not disprove the fact.

E. D. Cope.

ANTHROPOLOGY.[1]

Indian habitation in the Eastern United States.—Mr. Thomas Wilson of the Smithsonian Institution in a recent letter refering to a discussion in Washington as to the shape of Indian habitations east of the Mississippi, says, that while certain of the disputants "agreed that the Plains Indians of the present or modern times used wigwams made with poles fastened together at the top and spreading out in a circle at the bottom after the fashion of a Sibley tent, they

[1] This department is edited by Henry C. Mercer, University of Penna, Phila.

denied that any such structures were used by Indians, in the East. They insisted that these wigwams were confined to the plains and to the prairies and treeless countries, and did not exist, or were not found, and had never been used in the timbered countries—that in the timbered country Indian houses were made of wooden logs with upright sides and a flat or sloping roof. While I knew that many of these were made among the Iroquois of the East, and that this form was adopted in making the long houses (as they were called), I doubted whether they were so built among the nomadic and wandering tribes of Pennsylvania and the West Ohio, Indiana, etc. Can you give me any enlightenment thereon? If so, I will be obliged."

While it is not improbable that the shape of "wigwams," like burial customs varied considerably among the forest Indians, and while any camper out feels that a shelter often temporary, framed in the woods with available boughs, would vary in shape according to circumstances and suggest variation in more permanent structures, no one need hope to speak with final authority upon this subject, who has not ransacked the records of explorers, the narratives of individuals captured by Indians, the *relations* of the Jesuits, and the significant sketches of travellers in the last two centuries.

Dr. Daniel G. Brinton informs me that certain of the Brazilian forest Indians use the tepee form, and speaking of the Lenni Lenape, and quoting Nelson's History of New Jersey, writes: "William Penn describes the dwellings of the Delaware Indians as 'houses of mats, or barks of trees, set on poles, hardly higher than a man.' Pastorius states that 'young trees would be bent towards a common centre and the branches interlaced and fastened together as a frame work, and covered with bark.' Wassenaer says, 'they would construct a circular matted hut, with either angular or rounded top, thatched or lined with mats, a rent hole in the top serving for the escape of smoke.' This last description is strictly that of a tepee and shows that the angular pointed hut was in use by the Mohigan and Lenape Indians. Wassenaers' History is printed in Vol. III New York Documentary History."

The above quotation from Penn, however, if given correctly in Watsons' Annals of Philadelphia Vol. II. p., 153 reads distinctly against the tepee form. "Their houses were made of mats or barks of trees set on poles, *in the fashion of an English barn*, but out of the power of the winds for they are hardly higher than a man." And we find a rectangular structure again ascribed to the work of a band of Lenapes squatting in the suburbs of Philadelphia about 1770-80, in

Watson Vol. II. p. 31 where a person 80 years old in 1842 relates that he well remembers seeing colonies of Indians of twenty or thirty persons, often coming through the town (Germantown) and sitting down in Logan's woods, others in the present (1842) open field southeast of Griggs' place. They would make their huts and stay a whole year at a time and make and sell baskets, ladles and tolerably good fiddles. He has seen them shoot birds and young squirrels there with their bows and arrows. Their huts were made of four upright saplings with crotch limbs at top. The sides and tops were of cedar bushes and branches. In these they lived in the severest winters. Their fire was on the ground and in the middle of the area."

As the barn structure with its ridge pole would take six upright crotched saplings, this rectangle set up by half civilized indians with only four, was not barn shaped but single sloped like the simplest form of shed. The form described above by Pastorius judging from the tendency of elastic saplings when pulled together at the top to bow outward, would probably have resulted in a round roofed structure of the bee hive pattern if round at the base, or if rectangular, in such a building as De Brys' picture made in 1690 refers to Virginia Indians (Contributions to N. A. ethnology Vol. IV) or Captain John Smith carefully draws over the head of the sitting Powhatan in the upper left hand-corner of his map of Virginia (see Narr. and Critical History of Am., III, 166.) But if we believe Wassenaer who distinctly describes the Sioux Tepeè we must allow the latter form to the Delawares.

Too much importance need not be ascribed to the minute realistic outlines of habitations made to stand for Indian villages upon certain old maps drawn on a large scale as for instance in Dumont de Montigny's map of Louisiana (1746), when all Indian villages are marked with tepee like points from the Illinois River to New Orleans and from the Mobile to the Misissippi Rivers. On the other hand Du Prats, in a similar map (1758) gives the barn shape.[2] In other maps the structures seem too carefully and designedly drawn to be without archæoloical value. As when Father Abrahams Almanac Map 1761 (Narrative and Crit. Hist. V, 497) marks seven indian towns in the tepee shape near the junction of the Allegheny and Monongahela Rivers, and Hennepin in his map (1740) of the Mississippi valley and lakes (Narr. and Crit. Hist. IV 252 and 249) and again in his map of the lake region (1683) clearly shows pointed wigwams about the head waters of the Mississippi, as against small rectangular figures for the lower valley. Hawkins describes a communal Indian house seen in Florida as

[2] Narrative and Critical History of America Vol. V. p. 66.

like a great barn in strength, not inferior to ours. Lescarbot's map of Montreal 1609. (Narr. and Crit. His. IV 304) shows the palisaded Indian village of Hochelaga with barn-shaped round-roofed rectangular structures as in John Smith's cut, and in a map of Lake Ontario and the Iroquois Country 1662-63, (from one of the Jesuit *relations*) the indian villages are barn-shaped and with pointed roofs. La Hontain suggests the same shape in his map of the lake region 1709 (Narr. Crit. Hist. IV 281-261-258) and several Indian lodges of the circular bee-hive pattern surrounded by cultivated enclosures are given by Champlain in his map of Plymouth Harbor 1605. (Narr. and Crit. History IV 109). While not only the round bee-hive pattern, but also the long rectangle with round roof, as in Smith, are carefully drawn by the same explorer in his map of Nauset Harbor, 1604-05 (Land fall of Leif Erickson by Eben Norton Horsford p. 78).

More interesting is the direct evidence of the Indians themselves. The Lenape Stone, found in the Lenape region in 1872, and whose authenticity after ten years observation I have been unable to doubt, shows three pointed figures near trees, unmistakably referring to tepee shaped habitations in the right of the drawing, and another figure similarly outlined on the reverse, (See the Lenape Stone or the Indian and the Mammoth by H. C. Mercer, Putnam, N. Y. 1885). Another stone figured by me from the same locality. (See Lenape Stone p. 94) seems again to be inscribed with three tepee like forms.

No less explicit is the tepee figure upon the so called Winnipeseogee Stone found on the shores of Lake Winnipeseogee. (See Abbotts' Primitive Industry p. 362). George Copway (See Bureau of Ethnology Report 1888-89 p. 493 and 242) shows us Ojibway drawings which doubtless refer to the same pointed form of habitation.

That the sides of the barn shaped structures when built as by the Iroqois were invariably made of logs, is not to be supposed from the statement above quoted from Wm. Penn., and the drawing by Captain John Smith. All things considered, we have reason for supposing, subject to correction from documentary investigation, that though the barn shaped and round roofed rectangular structures were common, not only the bee hive, but the true tepee form were in use by Indians in the Pre-Columbian forest east of the Mississippi.

HENRY C. MERCER.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

Novia Scotian Institute of Science.—The 13th of April.—The following papers were read: Preliminary Notes on the Orthoptera of Nova Scotia. By Harry Piers, Esq. Notes on the Newt (D. *viridescens*) and on the Ring-Necked and Garter Snakes (D. *punctatus* and E. *sirtalis.*) By A. H. MacKay, Esq., LL. D., F. R. S. C., Superintendent of Education. On the Calculation of the Conductivity of Mixtures of Aqueous Solutions of Electrolytes having a common ion. By D. MacIntosh, Esq., Physical Laboratory, Dalhousie College.—HARRY PIERS, *Secretary.*

Boston Society of Natural History.—March 18th.—The following paper was read: Prof. Charles R. Cross, "The X rays." With experimental illustrations.

April 1st.—The following paper was read: Prof. William Libbey, "The Hawaiian Islands."

April 15th.—The following papers were read: Mr. M. L. Fuller, "A new occurrence of Carboniferous fossils in the Narragansett Basin. Prof. Alpheus Hyatt, "The evidence of the descent of man from the ape. A discussion upon the subject of Prof. Hyatt's followed, Prof. Thomas Dwight, Prof. C. S. Minot, and others participating.—SAMUEL HENSHAW, *Secretary.*

American Philosophical Society.—March 20th.—An obituary of Rev. W. H. Furness, by Jos. G. Rosengarten, was presented; Mrs. Cornelius Stevenson read a short paper on "Remains of Libyan Invaders of Egypt," discovered in 1895 by Mr. Flinders Petrie.

April 10.—Prof. Cope made some observations on the figures on a tablet from Nippur, pointing out the physical characters of the men and animals represented.

Academy of Natural Science, Philadelphia.—A meeting of the Anthropological Section was held the 13th of March.—The following papers were read: Prof. F. Edge Kavanagh, addressed the Section on "Right Handedness," was the subject discussed by Drs. Mills, Allen and Brinton, Professors Witmer, Culin, Jastrow and Gudeman.

Anthropological Section was held at the Academy on Friday, April 10th.—The following paper was read: Prof. Lightner Witmer on "Psycho-physical Measurement."—CHARLES MORRRIS.

New York Academy of Sciences.—Biological Section, March 9th.—Mr. F. B. Sumner read a paper on "The Descent Tree of the Variations of a Land Snail from the Philippines," illustrated by a lantern slide. Mr. Sumner described the range in variation in size and markings in the shell, and arranged the varieties in the form of a tree of three branches diverging from the most generalized type. It was shown that these several varieties occupy the same geographical region and Mr. Sumner was of the opinion that their occurrence could not be explained by natural selection since if the colorations were supposed to be protective it would be impossible to explain the evolution of these three types. Prof. Osborn, in discussion, was inclined to take the same view. Dr. Dyar, however, thought the explanation by natural selection not necessarily excluded, since the variations seemed analogous to the dimorphism in sphinx larvae, which has been shown by Poulton to be probably due to this factor.

The other paper was by Dr. Arnold Graf on "The Problem of the Transmission of Acquired Characters."

Dr. Graf discussed the views of the modern schools of evolutionists and adopted the view that the transmission of acquired characters must be admitted to occur. He cited several examples which seemed to support this view, and especially discussed the sucker in leeches as an adaptation to parasitism and the evolution of the chambered shell in a series of fossil Cephalopods.

Prof. Osborn remarked in criticism of Dr. Graf's paper that this statement does not appear to recognize the distinction between *ontogenic* and *phylogenic* variation, or that the adult from any organism is an exponent of the stirp, or constitution. The Environment. If the environment is normal the adult would be normal, but if the environment (which includes all the atmospheric, chemical, nutritive, motor and psychical circumstances under which the animal is reared) were to change, the adult would change correspondingly; and these changes would be so profound that in many cases it would appear as if the constitution, or stirp, had also changed. Illustrations might be given of changes of the most profound character induced by changes in either of the above factors of the environment, and in the case of the motor factor or animal motion, the habits of the animal might, in the course of a life time, profoundly modify its structure. For example, if the human infant were brought up in the branches of a tree as an arboreal type instead of as a terrestrial, bi-pedal type, there is little doubt that some of the well known early adaptations to arboreal habit (such as the turning in of the soles of the feet, and the grasping of the

hands) might be retained and cultivated, thus a profoundly different type of man would be produced. Similar changes in the action of environment are constantly in progress in nature since there is no doubt that the changes of environment and the new habits which it so brings about far outstrip all changes in constitution. This fact which has not been sufficiently emphasized before, offers an explanation of the evidence advanced by Cope and other writers that change in the forms of the skeletons of the vertebrates first appears in ontogeny and subsequently in phylogeny. During the enormously long period of time in which habits induced ontogenic variations it is possible for natural selection to work very slowly and gradually upon predispositions to useful correlated variations, and thus what are primarily *ontogenic variations* become slowly apparent as *phylogenic variations* or congential characters of the race.—C. L. BRISTOL, *Secretary.*

The Academy of Science of St. Louis.—March 16th.—Mr. Trelease presented some of the results of a recent study of the poplars of North America, made by him for the Systematic Botany of North America, and exhibited specimens of the several species and recognized varieties. Specimens were also exhibited of an apparently undescribed poplar from the mountains of northern Mexico, which he proposed to characterize shortly, and, for comparison, specimens of the two other species of poplar known to occur in Mexico, and of the European allies of the supposed new species, were laid before the Academy. The paper was discussed by Drs. Green, Glatfelter, and Kinner, Mr. Winslow, and Professor Kinealy.

The Academy, in co-operation with the joint committee of the scientific societies of Washington, adopted resolutions favoring the appointment of a permanent chief for the scientific work of the United States Department of Agriculture.

April 6th.—Prof. C. R. Sanger spoke on the commercial synthesis of acetylene, illustrating the flame procurable from this gas when burned with a proper proportion of air.

Prof. Sanger also presented the results of a preliminary biological and chemical examination into the ice supply of St. Louis, and exhibited a device for melting the ice in such examinations without danger of contamination from atmospheric ammonia, etc.

The Secretary presented for publication, by title, a paper by Mr. Charles Robertson, entitled "Flowers and Insects."

Mr. William H. Roever presented a paper on the geometry of the lines of force from an electrified body, in which it was shown that:

(a.) the curve representing a line of force proceeding from a system consisting of two parallel electrified lines, is the locus of the intersection of two straight lines, rotating in the same plane about these two parallel lines as axes with uniform but different angular velocities. (b.) the curve representing a line of force proceeding from a system consisting of two electrified points, is the locus of the intersection of two straight lines, rotating, in the same plane about parallel axes passing through those points, in such a manner that the versines of their angles of inclination to the plane of the axes change at uniform but different rates.

April 20th.—Dr. C. M. Woodward presented the results of a study of certain statistics of school attendance, from which it appeared that the average age of withdrawal from the public schools in three cities compared was as follows: Boston, 15.8; Chicago, 14.6; St. Louis, 13.7.

Professor J. H. Kinealy exhibited and gave a mathematical discussion of the Stang planimeter, an interesting and simple instrument of Danish invention, but improved in the United States.

WILLIAM TRELEASE, *Recording Secretary.*

U. S. National Academy of Sciences.—April 21, 1896.—The following papers were read: The Geological Efficacy of Alkali Carbonate Solutions, E. W. Hilgard; On the Color Relations of Atoms, Ions and Molecules, M. Carey Lea; On the Characters of the Otocœlidæ, E. D. Cope; Exhibition of a Linkage whose motion shows the Laws of Refraction of Light, A. M. Mayer; Location in Paris of the Dwelling of Malus, in which he made the discovery of the Polarization of Light by Reflection, A. M. Mayer; (1) On Experiments showing that the X-Rays cannot be Polarized by passing through Herapathite; (2) The Density of Herapathite; (3) Formulæ of Transmission of the X-Rays through Glass, Tourmaline and Herapathite, A. M. Mayer; On the X-Rays from a Statical Current produced by a Rapidly Revolving Leather Belt, W. A. Rogers and Frederick Brown; Biographical Memoir of James Edward Oliver, G. W. Hill; Biographical Memoir of Charles Henry Davis, C. H. Davis; Biographical Memoir of George Engelmann, C. A. White; Legislation Relating to Standards, T. C. Mendenhall; On the Determination of the Coefficient of Expansion of Jessop's Steel, between the limits of O° and 64° C., by the Interferential Method, E. W. Morley and W. A. Rogers; On the Separate Measurement, by the Interferential Method, of the Heating Effect of Pure Radiations and of an Envelope of Heated Air, W. A. Rogers; On the Logic of Quantity, C. S. Peirce; Judgement in Sensation and Perception, J. W. Powell; The Variability in Fermenting Power of the Colon

Bacillus under Different Conditions, A. W. Peckham (Presented by J. S. Billings); Experiments on the Reflection of the Röntgen Rays, O. N. Rood; Notes on Röntgen Rays, H. A. Rowland; Some Studies in Chemical Equilibrium, Ira Remsen; The Decomposition of Diazo-compounds by Alcohol, Ira Remsen; On Double Halides containing Organic Bases, Ira Remsen; Results of Researches of Forty Binary Stars, T. J. J. See; On a Remarkable New Family of Deep-sea Cephalopoda and its bearing on Molluscan Morphology, A. E. Verrill; The Question of the Molluscan Archetype, an Archi-mollusk, A. E. Verrill; On some Points in the Morphology and Phylogeny of the Gastropoda, A. E. Verrill; Source of X-Rays, A. A. Michelson and S. W. Stratton; The Relative Permeability of Magnesium and Aluminum to the Röntgen Rays, A. W. Wright; The State of Carbondioxide at the Critical Temperature, C. Barus; The Motion of a Submerged Thread of Mercury, C. Barus; On a Method of Obtaining Variable Capillary Apertures of Specified Diameter, C. Barus; On a New Type of Telescope Free from Secondary Color, C. S. Hastings; The Olindiadæ and other Medusæ, W. K. Brooks; Budding in Perophora, W. K. Brooks and George Lefevre; Anatomy of Yoldia, W. K. Brooks and Gilman Drew; On the *Pithecanthropus erectus* from the Tertiary of Java, O. C. Marsh.

C. D. Walcott and R. S. Woodward were elected members.

SCIENTIFIC NEWS.

Prof. Charles L. Edwards of the University of Cincinnati is to open a biological station this summer at Biscayne Bay, Florida. The place is well situated for the study of the tropical and sub-tropical flora and fauna, while its situation upon the continent makes it more readily accessible than the West India Islands. There will be opportunity for investigation while less mature students will have lectures and laboratory instructions. The session begins June 22d, and continues six weeks. A laboratory fee of $25,00 covers tuition, use of apparatus, reagents, etc., and Prof. Edwards estimates the total necessary expenses of each student, including board, railroad fares, etc., at from $100 to $125. It is also proposed to open a department of laboratory supply and to furnish all available material properly prepared at reasonable rates. For further information address Prof. Edwards at the University of Cincinnati.

Among the recent appointments to honorary membership in Learned Societies we notice, Sir W. H. Flower, by the Swedish Academy of Science; Prof. E. Ray Lankester, by the Russian Academy of Science; A. N. Beketow, Prof. Jas. Hall, Charles D. Walcott and Dr. G. Retzius by the St. Petersburg Academy of Science.

Dr. G. Lawson, botanist, of Halifax, N. S., died December 10th, 1895. It was owing to a confusion in names that the report of the death of the Canadian geologist, G. Dawson, arose.

The French Association for the Advancement of Science held its meeting this year at Tunis, from April 1 to 11. The Botanical Society of France, met at the same time and place.

Dr. George Baur, of the University of Chicago, will spend the summer in Munich, his former home, where he will study the rich paleontological collections of the University.

An expedition started, the middle of March to explore the interior of New Guinea. Dr. Lauterbach the leader takes charge of the botany, Dr. Kersting of the zoology.

The report of the death of the botanist K. Wilhelm, of Vienna is an error, caused by a confusion of names, his brother G. Wilhelm having died Nov. 30th, 1895.

Dr. H. M. Ward, of Cooper's Hill, England, accepts the Professorship of Botany in the University of Cambridge as successor to the late Professor Babbington.

Prof. K. G. Huefner, of Tübingen, has been called to the University of Strasburg where he succeeded the late Prof. Hoppe Seyler in the chair of Physiological Chemistry.

Prof. F. von Sandberger, who recently celebrated his fifty year Doctor-jubilee, has retired from the Professorship of Mineralogy in the University of Würzburg.

Prof. W. A. Locy, for several years Professor of Biology in Lake Forest University goes to Northwestern University, Evanston, Ill., as Professor of Zoology.

H. A. Miers, assistant keeper in the British Museum, goes to the University of Oxford as Professor of Mineralogy, succeeding the late Professor Maskelyne.

Dr. H. Schauinsland, of Bremen, has gone to the Island of Laysan for a ten month's exploring expedition, intending to study both the flora and fauna.

Dr. Looss, for several years docent in the University of Leipzig, has been advanced to the position of Extraordinary Professor.

Dr. E. Sickenberger, Professor of Botany and Chemistry in the medical school of Cairo, Egypt, died December 10th, 1895.

Dr. L. Edinger, of Frankfort, A. M. well known for his researches on the brain, has been honored with the title of Professor.

Dr. F. Saccardo, has been appointed Professor of Plant Pathology in the school of Oenology and Viticulture at Avellino.

Dr. P. Tauber, of Berlin, has sailed for South America intending to study the plants of Brazil, Venezuela and Guinea.

Dr. G. Wagener, Professor of Anatomy in the University of Marburg, died February 10th, 1896, at the age of 70.

Dr. F. Hochstetter, formerly of Vienna, goes to the University of Innsbruck, as ordinary Professor of Anatomy.

Dr. Katzer, has been elected Director of the Mineralogical-Geological section of the Museum of Para, Brazil.

Dr. L. Neumann has been appointed Ordinary Professor of Geography at the University of Freiberg.

Dr. E. Topsent, of Rheims, has been called to the chair of zoology in the Medical School at Rennes, France.

Dr. Seidentopf, of Bremen, has been appointed Assistant in Minerology in the University of Göttingen.

Dr. G. Horvath of Budapesth has been appointed Director of the Royal Hungarian Museum, zoological section.

Lieut H. E. Barnes, well known through his studies of Asiatic ornithology, died recently at the age of 48.

Dr. A. Schadmberg, an investigator of the flora and ethnology of the Philippines, died recently in Manila.

Count J. von Bergenstamn, the well known student of the Diptera, died January 31, 1896 in Vienna.

Dr. A. Zimmermann, becomes Private docent in Vegetable Physiology in the University of Berlin.

Dr. L. Buscalone, of Turin, goes to the University of Göttingen as Assistant in Plant Physiology.

G. C. Druce has been elected Custodian of the Fielding herbarium of the University of Oxford.

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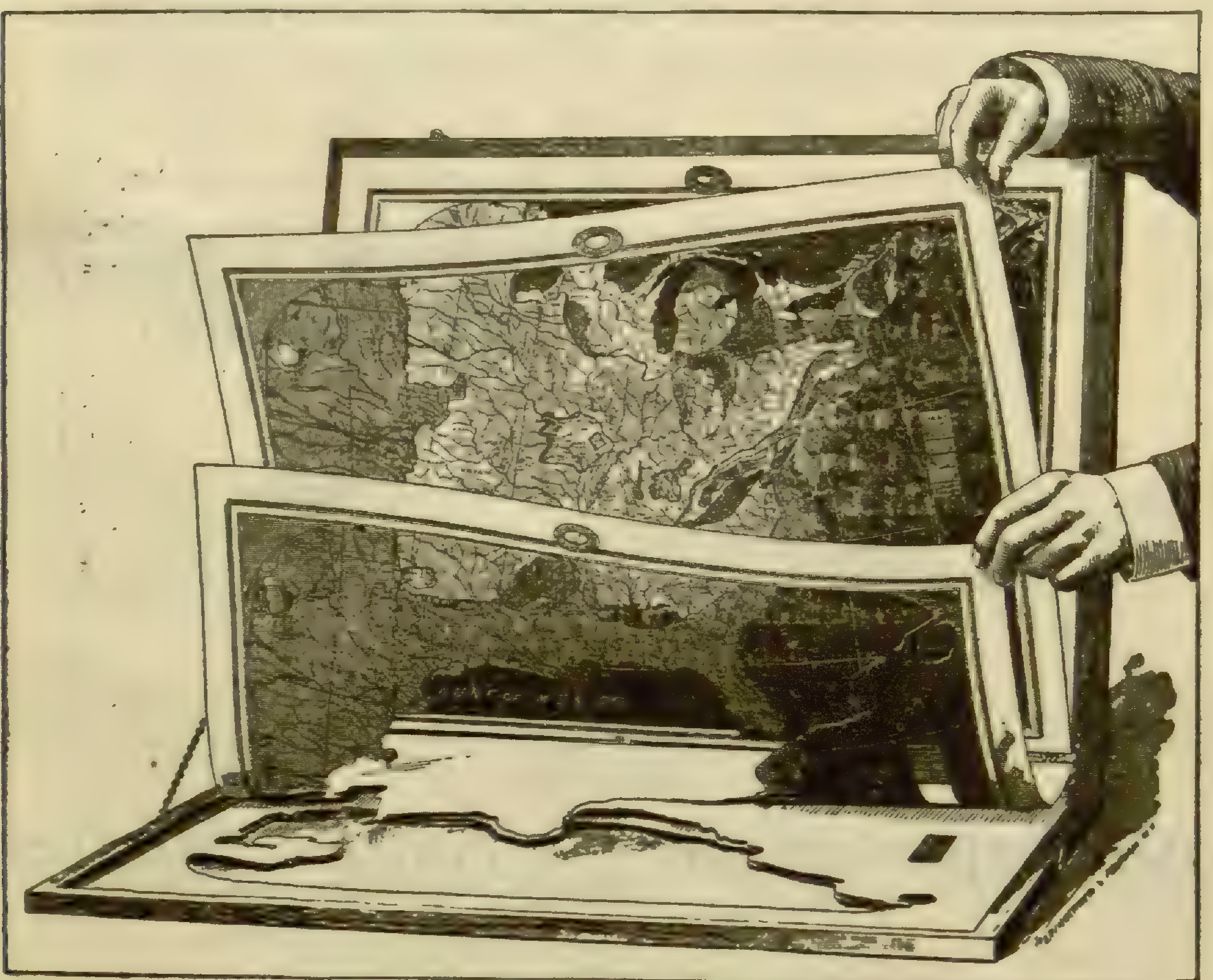

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Rev. Wm. C. Winslow, D. D., L. L. D., Egypt.
Prof. T. F. Wright, Explorations in Palestine.
Henry W. Haynes, Paleolithics and European Archaeology.
Dr. A. S. Gatschett, Indian Linguistics.
Marshall H. Seville, Mexico and Central America.
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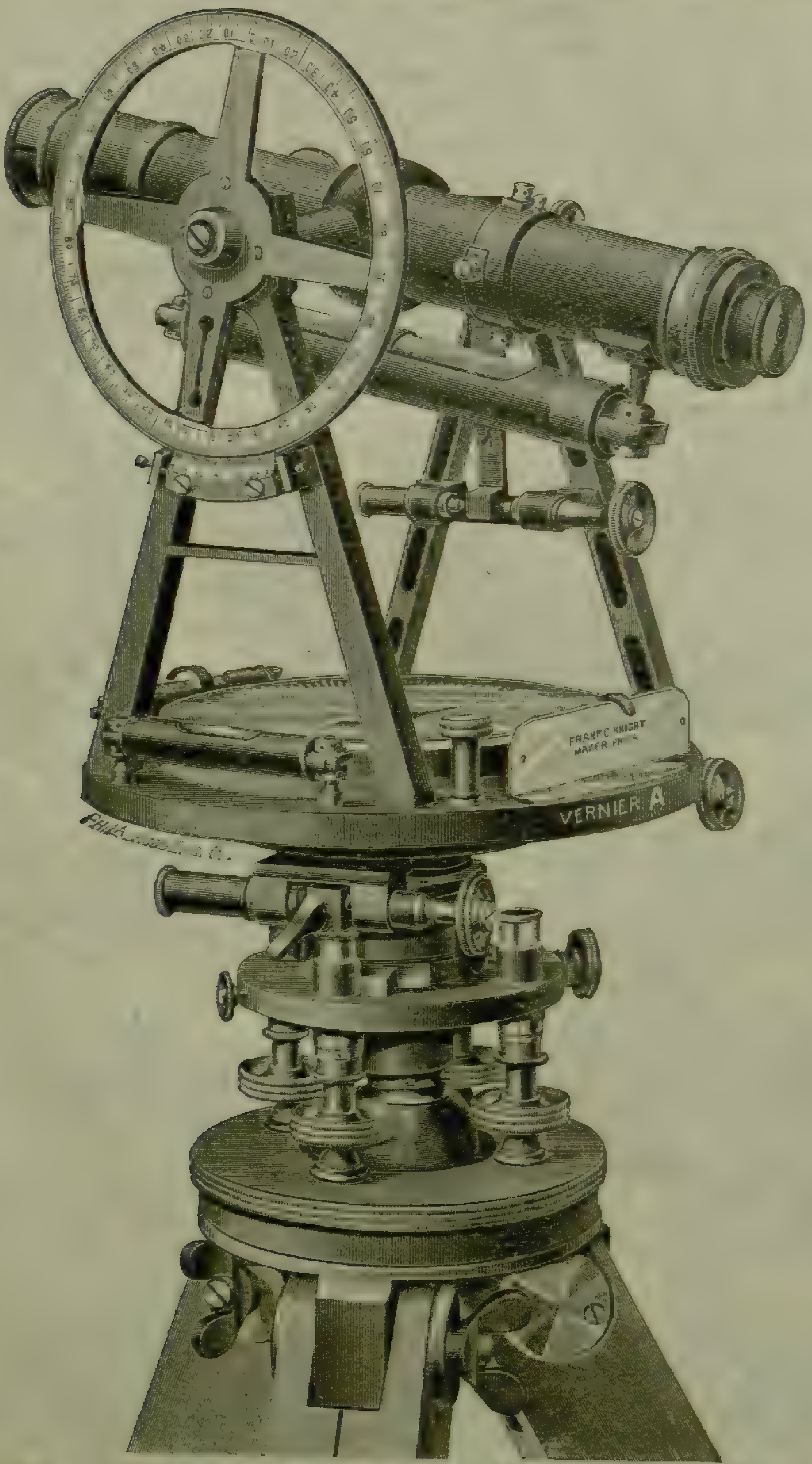

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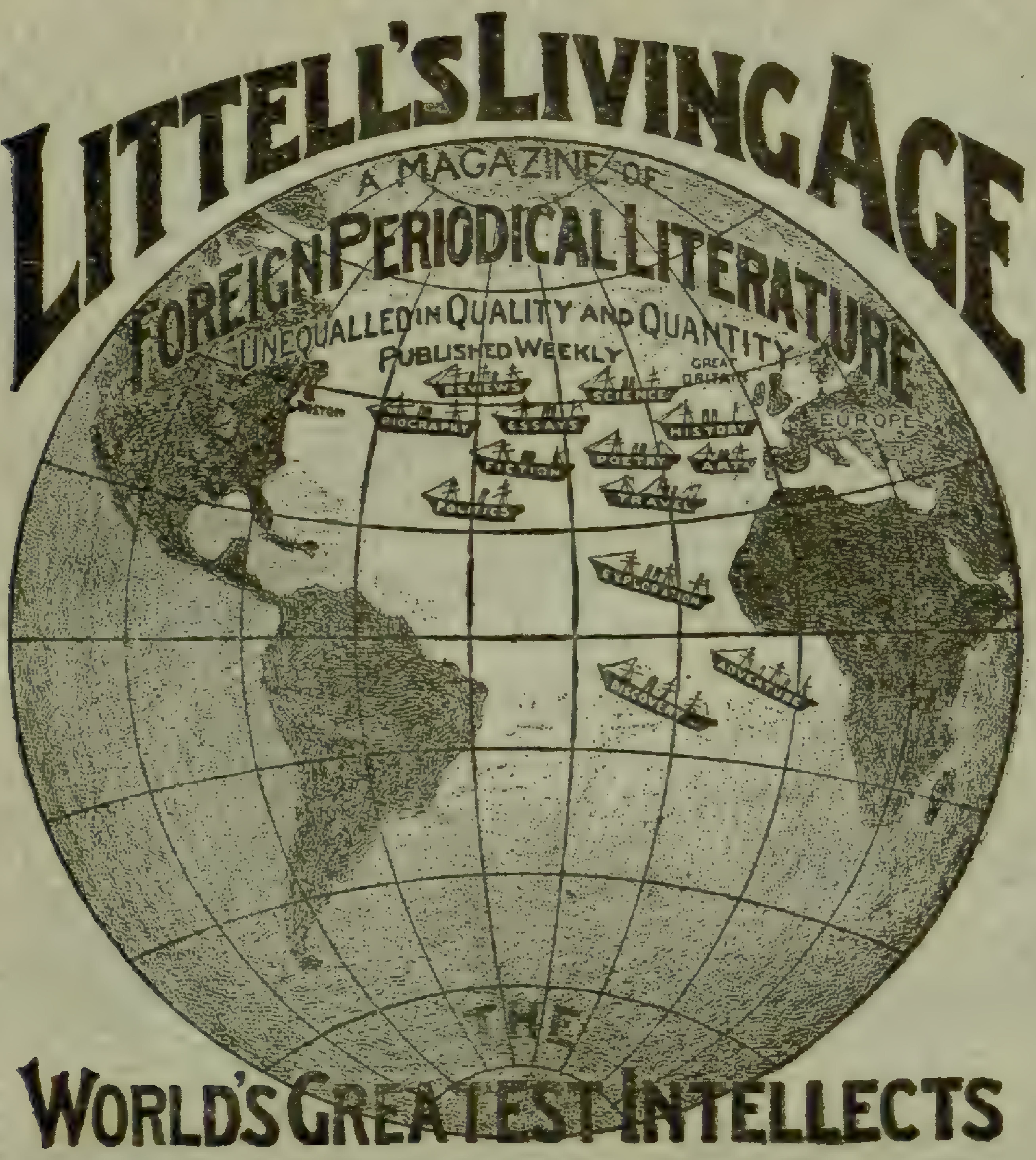

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THE

AMERICAN NATURALIST

VOL. XXX. June, 1896. 354

A NEW FACTOR IN EVOLUTION.

BY J. MARK BALDWIN.

In several recent publications I have developed, from different points of view, some considerations which tend to bring out a certain influence at work in organic evolution which I venture to call "a new factor." I give below a list of references[1] to these publications and shall refer to them by number as this paper proceeds. The object of the present paper is to

[1] References:

(1). *Imitation: a Chapter in the Natural History of Consciousness, Mind* (London), Jan., 1894. Citations from earlier papers will be found in this article and in the next reference.

(2). *Mental Development in the Child and the Race* (1st. ed., April, 1895; 2nd. ed., Oct., 1895; Macmillan & Co. The present paper expands an additional chapter (Chap. XVII) added in the German and French editions and to be incorporated in the third English edition.

(3). *Consciousness and Evolution, Science*, N. Y., August, 23, 1895; reprinted printed in the AMERICAN NATURALIST, April, 1896.

(4). *Heredity and Instinct* (I), *Science*, March 20, 1896. Discussion before N. Y. Acad. of Sci., Jan. 31, 1896.

(5). *Heredity and Instinct* (II), *Science*, April 10, 1896.

(6). *Physical and Social Heredity, Amer. Naturalist*, May, 1896.

(7). *Consciousness and Evolution, Psychol. Review*, May, 1896. Discussion before Amer. Psychol. Association, Dec. 28, 1895.

gather into one sketch an outline of the view of the process of development which these different publications have hinged upon.

The problems involved in a theory of organic development may be gathered up under three great heads: Ontogeny, Phylogeny, Heredity. The general consideration, the "factor" which I propose to bring out, is operative in the first instance, in the field of *Ontogeny*; I shall consequently speak first of the problem of Ontogeny, then of that of Phylogeny, in so far as the topic dealt with makes it necessary, then of that of Heredity, under the same limitation, and finally, give some definitions and conclusions.

I.

Ontogeny: "Organic Selection" (see ref. 2, chap. vii).—The series of facts which investigation in this field has to deal with are those of the individual creature's development; and two sorts of facts may be distinguished from the point of view of the *functions which an organism performs in the course of his life history.* There is, in the first place, the development of his heredity impulse, the unfolding of his heredity in the forms and functions which characterize his kind, together with the congenital variations which characterize the particular indiual—the phylogenetic variations, which are constitutional to him; and there is, in the second place, the series of functions, acts, etc., *which he learns to do himself in the course of his life.* All of these latter, the *special modifications which an organism undergoes during its ontogeny*, thrown together, have been called "acquired characters," and we may use that expression or adopt one recently suggested by Osborn,[2] "ontogenic variations" (except that I should prefer the form "ontogenetic variations"), if the word variations seems appropriate at all.

[2] Reported in *Science*, April 3rd.; also used by him before N. Y. Acad. of Sci., April 13th. There is some confusion between the two terminations "genic" and "genetic." I think the proper distinction is that which reserves the former, "genic," for application in cases in which the word to which it is affixed qualifies a term used *actively*, while the other, "genetic" conveys similarly a *passive* signification; thus agencies, causes, influences, etc., and "ontogenic phylogenic, etc.," while effects, consequences, etc, and "ontogenetic, phylogenetic, etc."

Assuming that there are such new or modified functions, in the first instance, and such "acquired characters," arising by the law of "use and disuse" from these new functions, our farther question is about them. And the question is this: How does an organism come to be modified during its life history?

In answer to this question we find that there are three different sorts of ontogenic agencies which should be distinguished—each of which works to produce ontogenetic modifications, adaptations, or variations. These are: first, the physical agencies and influences in the environment which work upon the organism to produce modifications of its form and functions. They include all chemical agents, strains, contacts, hindrances to growth, temperature changes, etc. As far as these forces work changes in the organism, the changes may be considered largely "fortuitous" or accidental. Considering the forces which produce them I propose to call them "physico-genetic." Spencer's theory of ontogenetic development rests largely upon the occurrence of lucky movements brought out by such accidental influences. Second, there is a class of modifications which arise from the spontaneous activities of the organism itself in the carrying out of its normal congenital functions. These variations and adaptations are seen in a remarkable way in plants, in unicellular creatures, in very young children. There seems to be a readiness and capacity on the part of the organism to "rise to the occasion," as it were, and make gain out of the circumstances of its life. The facts have been put in evidence (for plants) by Henslow, Pfeffer, Sachs; (for micro-organisms) by Binet, Bunge; (in human pathology) by Bernheim, Janet; (in children) by Baldwin (ref. 2, chap. vi.) (See citations in ref. 2, chap. ix, and in Orr, *Theory of Development*, chap. iv). These changes I propose to call "neuro-genetic," laying emphasis on what is called by Romanes, Morgan and others, the "selective property" of the nervous system, and of life generally. Third, there is the great series of adaptations secured by conscious agency, which we may throw together as "psycho-genetic." The processes involved here are all classed broadly under the term "intelligent," i. e., imitation, gregarious influences, maternal in-

struction, the lessons of pleasure and pain, and of experience generally, and reasoning from means to ends, etc.

We reach, therefore, the following scheme:

Ontogenetic Modifications.	*Ontogenic Agencies.*
1. Physico-genetic.	1. Mechanical.
2. Neuro-genetic.	2. Nervous.
3. Psycho-genetic.	3. Intelligent.
	Imitation.
	Pleasure and pain.
	Reasoning.

Now it is evident that there are two very distinct questions which come up as soon as we admit modifications of function and of structure in ontogenetic development: first, there is the question as to how these modifications can come to be adaptive in the life of the individual creature. Or in other words: What is the method of the individual's growth and adaptation as shown in the well known law of "use and disuse?" Looked at functionally, we see that the organism manages somehow to accommodate itself to conditions which are favorable, to repeat movements which are adaptive, and so to grow by the principle of use. This involves some sort of selection, from the actual ontogenetic variations, of certain ones—certain functions, etc. Certain other possible and actual functions and structures decay from disuse. Whatever the method of doing this may be, we may simply, at this point, claim the law of use and disuse, as applicable in ontogenetic development, and apply the phrase, "Organic Selection," to the organism's behavior in acquiring new modes or modifications of adaptive function with its influence of structure. The question of the method of "Organic Selection" is taken up below (IV); here, I may repeat, we simply assume what every one admits in some form, that such adaptations of function—"accommodations" the psychologist calls them, the processes of learning new movements, etc.—*do occur.* We then reach another question, second; what place these adaptations have in the general theory of development.

Effects of Organic Selection.—First, we may note the results of this principle in the creature's own private life.

1. *By securing adaptations, accommodations, in special circumstances the creature is kept alive* (ref. 2, 1st ed., pp. 172 ff.). This is true in all the three spheres of ontogenetic variation distinguished in the table above. The creatures which can stand the "storm and stress" of the physical influences of the environment, and of the changes which occur in the environment, *by undergoing modifications of their congenital functions or of the structures which they get congenitally—these creatures will live; while those which cannot, will not.* In the sphere of neurogenetic variations we find a superb series of adaptations by lower as well as higher organisms during the course of ontogenetic development (ref. 2, chap. ix). And in the highest sphere, that of intelligence (including the phenomena of consciousness of all kinds, experience of pleasure and pain, imitation, etc.), we find individual accommodations on the tremendous scale which culminates in the skilful performances of human volition, invention, etc. The progress of the child in all the learning processes which lead him on to be a man, just illustrates this higher form of ontogenetic adaptation (ref. 2, chap. x–xiii).

All these instances are associated in the higher organisms, and all of them unite to *keep the creature alive.*

2. By this means *those congenital or phylogenetic variations are kept in existence, which lend themselves to intelligent, imitative, adaptive, and mechanical modification during the lifetime of the creatures which have them.* Other congenital variations are not thus kept in existence. So there arises a more or less widespread series of *determinate variations in each generation's ontogenesis* (ref. 3, 4, 5).[3]

[3] "It is necessary to consider further how certain reactions of one single organism can be selected so as to adapt the organism better and give it a life history. Let us at the outset call this process "Organic Selection" in contrast with the Natural Selection of whole organisms. . . . If this (natural selection) worked alone, every change in the environment would weed out all life except those organisms, which by accidental variation reacted already in the way demanded by the changed conditions—in every case new organisms showing variations, not, in any case, new elements of life-history in the old organisms. In order to the latter we would have to conceive . . . some modification of the old reactions in an organism through the influence of new conditions. . . . We are, accordingly, left to the view that the new stimulations brought by changes in the environment

The further applications of the principle lead us over into the field of our second question, i. e., phylogeny.

II.

Phylogeny: Physical Heredity.—The question of phylogenetic development considered apart, in so far as may be, from that of heredity, is the question as to what the factors really are which show themselves in evolutionary progress from generation to generation. The most important series of facts recently brought to light are those which show what is called "determinate variation" from one generation to another. This has been insisted on by the paleontologists. Of the two current theories of heredity, only one, Neo-Lamarkism—by means of its principle of the inheritance of acquired characters—has been able to account for this fact of determinate phylogenetic change. Weismann admits the inadequacy of the principle of natural selection, as operative on rival organisms, to explain variations when they are wanted or, as he puts it, "the right variations in the right place" (*Monist*, Jan., '96).

I have argued, however, in detail that the assumption of determinate variations of function in ontogenesis, under the principle of neurogenetic and psychogenetic adaptation, does away with the need of appealing to the Lamarkian factor. In the case i. g., of instincts, "if we do not assume consciousness, then natural selection is inadequate; but if we do assume consciousness, then the inheritance of acquired characters is unnecessary" (ref. 5).

"The intelligence which is appealed to, to take the place of instinct and to give rise to it, uses just these partial variations which tend in the direction of the instinct; so the intelligence *supplements* such partial co-ordinations, makes them functional, and *so keeps the creature alive.* In the phrase of Prof.

themselves modify the reactions of an organism. . . . The facts show that individual organisms do acquire new adaptations in their lifetime, and that is our first problem. If in solving it we find a principle which may also serve as a principle of race-development, then we may possibly use it against the 'all sufficiency of natural selection' or in its support" (ref. 2, 1st. ed., pp. 175-6.)

Lloyd Morgan, this prevents the 'incidence of natural selection.' So the supposition that intelligence is operative turns out to be just the supposition which makes use-inheritance unnecessary. Thus kept alive, the species has all the time necessary to perfect the variations required by a complete instinct. And when we bear in mind that the variation required is not on the muscular side to any great extent, but in the central brain connections, and is a slight variation for functional purposes at the best, the hypothesis of use-inheritance becomes not only unnecessary, but to my mind quite superfluous" (ref. 4, p. 439). And for adaptations generally, "the most plastic individuals will be preserved to do the advantageous things for which their variations show them to be the most fit, and the next generation will show an emphasis of just this direction in its variations" (ref. 3, p. 221).

We get, therefore, from Organic Selection, certain results in the sphere of phylogeny:

1. *This principle secures by survival certain lines of determinate phylogenetic variation in the directions of the determinate ontogenetic adaptations of the earlier generation.* The variations which were utilized for ontogenetic adaptation in the earlier generation, being thus kept in existence, are utilized more widely in the subsequent generation (ref. 3, 4). "Congenital variations, on the one hand, are kept alive and made effective by their use for adaptations in the life of the individual; and, on the other hand, adaptations become congenital by further progress and refinement of variation in the same lines of function as those which their acquisition by the individual called into play. But there is no need in either case to assume the Lamarkian factor" (ref. 3). And in cases of conscious adaptation: "We reach a point of view which gives to organic evolution a sort of intelligent direction after all; for of all the variations tending in the direction of an adaptation, but inadequate to its complete performance, *only those will be supplemented and kept alive which the intelligence ratifies and uses.* The principle of 'selective value' applies to the others or to some of them. So natural selection kills off the others; and the *future*

development at each stage of a species' development must be in the directions thus ratified by intelligence. So also with imitation. Only those imitative actions of a creature which are useful to him will survive in the species, for in so far as he imitates actions which are injurious he will aid natural selection in killing himself off. So intelligence, and the imitation which copies it, will set the direction of the development of the complex instincts even on the Neo-Darwinian theory; and in this sense we may say that consciousness is a 'factor'" (ref. 4).

2. *The mean of phylogenetic variation being thus made more determinate, further phylogenetic variations follow about this mean, and these variations are again utilized by Organic Selection for ontogenetic adaptation.* So there is continual phylogenetic progress in the directions set by ontogenetic adaptation (ref. 3, 4, 5). "The intelligence supplements slight co-adaptations and so gives them selective value; but it does not keep them from getting farther selective value as instincts, reflexes, etc., by farther variation" (ref. 5). "The imitative function, by using muscular co-ordinations, supplements them, secures adaptations, keeps the creature alive, prevents the 'incidence of natural selection,' and so gives the species all the time necessary to get the variations required for the full instinctive performance of the function" (ref. 4). But, "Conscious imitation, while it prevents the incidence of natural selection, as has been seen, and so keeps alive the creatures which have no instincts for the performance of the actions required, nevertheless does not subserve the utilities which the special instincts do, nor prevent them from having the selective value of which Romanes speaks. Accordingly, on the more general definition of intelligence, which includes in it all conscious imitation, use of maternal instruction, and that sort of thing—no less than on the more special definition—we still find the principal of natural selection operative" (ref. 5).

3. *This completely disposes of the Lamarkian factor as far as two lines of evidence for it are concerned.* First, the evidence drawn from function, "use and disuse," is discredited; since by "organic selection," the reappearance, in subsequent generations, of the variations first secured in ontogenesis is ac-

counted for without the inheritance of acquired characters. So also the evidence drawn from paleontology which cites progressive variations resting on functional use and disuse. Second, the evidence drawn from the facts of "determinate variations;" since by this principle we have the preservation of such variations in phylogeny without the inheritance of acquired characters.

4. *But this is not Preformism in the old sense; since the adpatations made in ontogenetic development which "set" the direction of evolution are novelties of function in whole or part* (although they utilize congenital variations of structure). And it is only by the exercise of these novel functions that the creatures are kept alive to propagate and thus produce further variations of structure which may in time make the whole function, with its adequate structure, congenital. Romanes' argument from "partial co-adaptations" and "selective value," seem to hold in the case of reflex and instinctive functions (ref. 4, 5), as against the old preformist or Weismannist view, although the operation of Organic Selection, as now explained, renders them ineffective when urged in support of Lamarkism. "We may imagine creatures, whose hands were used for holding only with the thumb and fingers on the same side of the object held, to have first discovered, under stress of circumstances and with variations which permitted the further adaptation, how to make use of the thumb for grasping opposite to the fingers, as we now do. Then let us suppose that this proved of such utility that all the young that did not do it were killed off; the next generation following would be plastic, intelligent, or imitative, enough to do it also. They would use the same co-ordinations and prevent natural selection getting its operation on them; and so instinctive 'thumb-grasping' might be waited for indefinitely by the species and then be got as an instinct altogether apart from use-inheritance" (ref. 4). "I have cited 'thumb-grasping' because we can see in the child the anticipation, by intelligence and imitation, of the use of the thumb for the adaptation which the Simian probably gets entirely by instinct, and which I think an isolated and weak-minded child, say, would also come to do by instinct'" (ref. 4).

5. It seems to me also—though I hardly dare venture into a field belonging so strictly to the technical biologist—that *this principle might not only explain many cases of widespread "determinate variations" appearing suddenly, let us say, in fossil deposits, but the fact that variations seem often to be "discontinuous."* Suppose, for example, certain animals, varying, in respect to a certain quality, from *a* to *n* about a mean *x*. The mean *x* would be the case most likely to be preserved in fossil form (seeing that there are vastly more of them). Now suppose a sweeping change in the environment, in such a way that only the variations lying near the extreme *n* can accommodate to it and live to reproduce. The next generation would then show variations about the mean *n*. And the chances of fossils from this generation, and the subsequent ones, would be of creatures approximating *n*. Here would be a great discontinuity in the chain and also a widespread prevalence of these variations in a set direction. This seems especially evident when we consider that the paleontologist does not deal with successive generations, but with widely remote periods, and the smallest lapse of time which he can take cognizance of is long enough to give the new mean of variation, *n*, a lot of generations in which to multiply and deposit its representative fossils. Of course, this would be only the action of natural selection upon "preformed" variations in those cases which did not involve positive changes, in structure and function, *acquired in ontogenesis*; but in so far as such ontogenetic adaptations were actually there, the extent of difference of the *n* mean from the *x* mean would be greater, and hence the resources of explanation, both of the sudden prevalence of the new type and of its discontinuity from the earlier, would be much increased. This additional resource, then, is due to the "Organic Selection" factor.

We seem to be able also to utilize all the evidence usually cited for the functional origin of specific characters and groupings of characters. So far as the Lamarkians have a strong case here, it remains as strong if Organic Selection be substituted for the "inheritance of acquired characters." This is especially true where intelligent and imitative adaptations are

involved, as in the case of instinct. This "may give the reason, e. g., that instincts are so often coterminous with the limits of species. Similar structures find the similar uses for their intelligence, and they also find the same imitative actions to be to their advantage. So the interaction of these conscious factors with natural selection brings it about that the structural definition which represents species, and the functional definition which represents instinct, largely keep to the same lines" (ref. 5).

6. It seems proper, therefore, to call the influence of Organic Selection "a new factor;" for it gives a method of deriving the determinate gains of phylogeny from the adaptations of ontogeny without holding to either of the two current theories. *The ontogenetic adaptations are really new, not performed; and they are really reproduced in succeeding generations, although not physically inherited.*

(*To be continued.*)

THE PATH OF THE WATER CURRENT IN CUCUMBER PLANTS.

By Erwin F. Smith.

(*Continued from page 378*).

2. Upward Movement of One Per Cent. Eosine Water Through Cut Stems Plugged with Gelatine.

In all of these experiments a somewhat stiff gelatine was used (15 per cent.) to secure a relatively high melting point (about 27° C.) and this was tinged with India ink, so that the location of the gelatine plugs inside of the vessels could be determined accurately on cross section. Both substances being as far as has been determined inert to the plant, it is not likely that they could have in any way injured the carrying capacity of the walls of the vessels.[2]

[2]Recently Dixon and Joly (Annals of Botany, Sept., 1895, p. 403) have raised some objections to this view, but it cannot be said that they have fully established their case.

(No. 5). A much branched large vine, bearing many leaves, at least 60, the breadth of the best ones being 17 cm. The distance from the cut stem to the extremity of the longest shoot measured 218 centimeters. March 20, 1:52 p. m. The basal part of the stem was plunged into gelatine at 45° C., severed smoothly and left 40 minutes, the temperature of the gelatine when the cut stem was removed being 34° C. At 1:30 p. m. the dry bulb registered 18° C, and the wet bulb 16.5° C., and transpiration during the afternoon was probably not very active. On removing the cut stem from the melted gelatine, it was immediately plunged into water at 16° C., and kept there 10 minutes, i. e., until the gelatine was congealed. At 2:38 p. m. the stem was shortened about 3 millimeters and plunged into 1 per cent eosine water at a temperature of 16° C. A careful examination of the 3 millimeter segment showed that by far the larger number of the vessels of the stem (nearly all) were full of the black gelatine, but for unknown reasons, some of the spirals and a very few of the larger pitted vessels were not filled. 4:10 p. m. No trace of color in the veins of any of the leaves. 4:50 p. m. Not a trace of color in any of the leaves. The stem has now been in the eosine water 2 hours and 12 minutes. March 21, 11:30 a. m. The house is dryer than yesterday, and the demand on the plant for water is enormous. The foliage is shriveled or flabby, including the petioles,and hangs down, but not a trace of eosine is to be seen anywhere in any of the leaves, although the lowest leaf is within 24 centimeters of the cut end. There has also been no perceptible lowering of the level of the eosine water in which the stem rests. The sun shines hot through the glass, the temperature in the shade on a level with the bench being 24° C., while in the sun, four inches above, it is 29° C. 12:20 p. m. No trace of eosine visible externally in any part of stem or leaves, although it is nearly 22 hours since the stem was plunged into the stain. The stem was now removed and cut for examination with the following results: 2 cm. up.—There is a trifling stain. Nearly all of the vessels are full of gelatine, and in some of the bundles the stain shows *only in those spirals which did not fill.* 4 cm.

up.—Most of the vessels are full of gelatine. 8 cm. up.—About one-third of the vessels are full. 12 cm. up.—No gelatine in the vessels. Three of the nine bundles show no trace of stain. There is no fluid in the lumen of any of the vessels of the other 6 bundles, but the walls of the vessels and connecting tissues are partially stained in 4 bundles, and entirely in 2.30 cm. up, i. e., above several nodes.—The walls of a part of the vessels of each bundle are tinged with the stain, but less strongly than at 12 centimeters. The lignified parts of all of the spiral vessels show the stain, but part of the pitted vessels are free from stain, and all of the phloem, fundamental tissue, collenchyma and sclerenchyma. It would seem as if the spirals brought up the stain (the almost inappreciable quantity which passed by the gelatine plugs), and that from these it diffused out into the rest of the xylem. When the walls of the pitted vessels were stained, those of the connecting tracheids were also stained. 50 cm. up.—The red color is restricted to 4 inner and 2 outer bundles. In one of the two outer bundles and in all of the inner ones, the stain is confined to the region of the spirals and is barely visible in these. Longitudinal and oblique cuts also show in a striking way the restriction of the stain to the spirals. 60 cm. up.—Barest trace of stain in 3 bundles. 65 cm. up.—Only slightest trace of stain, restricted to the spirals of 2 bundles. 70 cm. up.—No trace of stain. 80 cm. up.—No trace of color. Two branches were given off just under the 50 cm. cut: In the lower there was a trace of stain in two bundles at 5 cm. from the main stem; in the the other there was no trace of the stain, 2 cm. or 5 cm. out.

Here there was every opportunity for water to pass through the walls of the vessels, the osmotic pull probably amounting to a pressure of several atmospheres, but there is no conclusive evidence that even a single drop passed up in this way. The very slight amount of eosine which passed up the stem may have gone through the walls of the vessels in obedience to the law of surface tension or may have passed through the lumina, owing to the incomplete plugging of some of the vessels, those which showed no gelatine being probably plugged by air and inoperative.

(No. 15). An old, much branched vine which has borne fruit and is nearly past profitable culture. The principal stem is 200 centimetres long (measured from the cut near the earth); the longest branch is 105 cm.; the next longest is 65 cm. The vine has lost much of its foliage but bears about 60 medium sized leaves (10 to 15 cm. broad) and as many more smaller ones, mostly from short, lateral branches, so that the transpiration on a sunny, windy day, like this, must be very considerable. March 22, 2:05 p. m. The base of the vine, which had previously been dug up carefully by the roots, and put at once into water, was cut 30 cm. above the roots under gelatine at a temperature of 40° C. 2:30 p. m. Many of the leaves have begun to wilt, showing that the transpiration and negative pressure must be very great. The stem was now shortened under the gelatine one centimeter and the segment examined. Most of the vessels were full of gelatine but not all. A dozen or so of the pitted vessels were empty and more than that many spirals. The vessels of some bundles appeared to be completely full including the spirals. 2:40 p. m. The foliage now shows a decided droop. Stem cut again under the still fluid gelatine 4 cm. up. Fully one-third of the pitted vessels are free from gelatine (contain air), but most of the spirals seem to be full. The torn central stem cavity is also full of gelatine and was in those examined yesterday. 3:30 p. m. Marked droop of all the foliage. Stem removed quickly from the gelatine which has been kept at 40° C. and plunged into water at 19° C. 3:50 p. m. Stem shortened slightly and put at once into 1 per cent eosine water cooled down to 14° C. An examination of the segment just removed shows that nearly all of the vessels are full of the solidified gelatine, but not *all*. 4:10 p. m. No trace of stain in any of the leaves, although those nearest the cut are only 15 centimeters up. 5:00 p. m. The vine is drooping and needs water badly, but can get none either through the walls of the vessels or through the gelatine plugs. An hour and ten minutes has passed since the stem was plunged into the eosine and yet there is not a trace of stain in any leaf, although the eosine water would have gone to the end of the vine and been dis-

tinctly visible in the veins of every leaf in 15 minutes but for the gelatine plugs, as we have seen from the preceding experiments. March 23, 11:00 a. m. Two-thirds of all the foliage of this vine is now dry-shriveled, and the remainder is very flabby, but there is not a trace of eosine visible in any of the leaves, not even in those which are near the cut end and still living. Sun shining, hot, some wind outside. Temperature in the shade, 6 inches above the bench, 27° C.; in sun, 30° C.; dry bulb, 26.5° C.; wet bulb, 22° C. Active transpiration. 1:45 p. m. Nine-tenths of the foliage is crisp-dry. No trace of color in any part of the stem or foliage. The stem was now removed from the fluid and cut for examination with the following results: One-half centimeter up.—Most of the pitted vessels were full of the black gelatine, one showed a rim of gelatine with a central air bubble. Spirals mostly not full. Diffuse stain in the parenchyma. The stain has passed through the gelatine itself in many instances (owing perhaps to its liquefaction on a. m. of March 23, when a beaker of water, in which the eosine bottle rested, became lukewarm and a beaker of gelatine on the bench near by became fluid). Six cm. up.—Comparatively few pitted vessels have gelatine in them; some of these are full, others have only a rim of gelatine around a succession of air bubbles. Eight cm. up.—The torn central stem cavity, which is still visible, contains no gelatine. A few pitted vessels contain gelatine mostly as rims around the walls, air being in the center. Ten cm. up.—No gelatine. Stain very feeble, diffused somewhat into the parenchyma. About ¼ of the pitted vessels unstained; color restricted to the spiral vessels in one bundle. Twenty cm. up.—No gelatine. Stain slight, not entirely restricted to the bundles but diffused out into the parenchyma on one side of the stem. Forty-three cm. up.—Slight traces of stain in 7 bundles, restricted to the spirals in 4; in two other bundles, only the outer angle of the xylem wedge is stained, on one side or the other, *i. e.*, those vessels which frequently fill with bacteria in advance of the rest of the pitted vessels, when the plant suffers from cucumber wilt. Seventy-five cm. up.—Stain restricted to six bundles, and very slight; confined to the

spirals in three bundles and almost so in a fourth. Several nodes have been cut. The stain is deepest in these parts of the stem and more widespread in the walls of the pitted vessels. Ninety cm. up.—There is still a trace of stain in three bundles; in one it is restricted to the walls of the spiral vessels, in the other two it does not occur in the spirals but on one side of the xylem, midway out in one, and in the outer angle of the other. Some of the branches were also examined. The branch 65 cm. long, separated from the parent shoot only ten centimeters above the cut surface. At 20 cm. from its junction with the main shoot, 7 bundles showed a trace of stain in the xylem part; in two of these the stain was restricted to the spirals, and in the other five it seemed to have diffused outward from the spirals into the neighboring pitted vessels. Twenty-three cm. up.—Only slightest trace of stain, restricted to the spirals of one bundle. Twenty-five cm. up.—Not a trace of stain. Thë branch 105 cm. long, separated from the parent shoot 41 centimeters above the cut surface. It was first cut at the junction with the main stem. Here the stain was to be seen in 7 bundles, but very slight and almost wholly restricted to the spiral vessels. Thirty cm. from the junction, *i. e.*, past several nodes.—Stain in xylem of all of the nine bundles; restricted to the spirals in 6, diffused in 3. This cut was made just above a node. The stain appears to be more restricted to the spirals in the internodes than in the nodes. Vessels empty. Sixty cm. up.—Only the slightest trace of stain in one bundle, so slight as to be readily overlooked. Sixty-five cm. up.—Color still present but so slight that no one would recognize it unless informed that stain had passed through the stem. Seventy cm. up.—All trace of the stain has disappeared.

It will be remembered that this vine was in the 1 per cent eosine water nearly 24 hours, during which time there was no perceptible lowering of the liquid, consequently all of the internal stain is readily accounted for, especially when we remember the powerful tinctive character of eosine, by the few drops of stain which managed to get past the gelatine plugs. We are warranted, therefore, in concluding that not a drop

passed up through the walls of the vessels, or if this be too strong a statement we are at least safe in saying that not enough passed up the walls to serve even most inadequately, the transpiration purposes of a single leaf, and this, perhaps, is the better forms in which to leave the statement. Neither can it be said, by way of objection that the eosine behaved differently from ordinary water for we have already seen that 1 per cent eosine water passes up unplugged stems readily, even for days and long after the stem is dead. In this case, judging from the state of the atmosphere, the temperature, and the amount of transpiring surface, (approximately 1,500 sq. cm.) at least 25 and probably 50 cubic centimeters would have been taken up by the plant in the first 24 hours but for the gelatine plugs. No explanation is open, therefore, except that the transpiration water passes up through the lumen of the vessels in the stem of the cucumber, and presumably in all other stems of similar structure, unless we assume that the gelatine passed into the walls of the vessels and destroyed their conductive power, and no one has proved this to be possible or even set forth facts rendering it probable. Considering the fact that the walls of the vessels in most plants are solid lignified structures and that the vessels are long open tubes, comparable to water pipes and in many plants probably continuous through the whole length of the stem, it would seem strange that this other view, viz. that the water passes upward through the wall itself and not through the lumen of the tube, should have ever gained credence, did we not know how often, even in science, the weight of a great name carries everything before it.

(To be Continued.)

EXTENSIVE MIGRATION IN BIRDS AS A CHECK UPON THE PRODUCTION OF GEOGRAPHICAL VARIETIES.

BY THOMAS H. MONTGOMERY, JR.

Two problems have of late years received much attention from ornithologists, and deservedly, namely, the faunal distribution of species, and their ranges of migration. But to my knowledge, no one has raised the question of the possible existence of a relation between the extent of the periodic migration and the amount of geographical variation evinced by a species. The object of this paper then is to show that such a relation does exist, that extensive migration tends to act as a check upon the production of geographical varieties, or races so-called.

In the first place, on comparing the amount of faunal variation with the extent of the periodical migrations in a given species, it will be found to be usually, if not always, the case, that those species which undertake migrations of more than the average extent—migrating through 30° or more of latitude, have no tendency to give rise to geographical varieties. In order to see how far this law extends, and whether exceptions to it may be found, I have compared all the species of North American birds with regard to this relation existing between variation and range of migration, using Ridgway's excellent "Manual of N. A. Birds" as my authority for the amount of variation and extent of migration in these species. For our present purposes the North American species may be divided into three groups, based on the extent of their migrations: (1) species with exceedingly protracted migrations, but irregular as to the localities traversed; (2) species with more or less regular migrations, of 30° lat. or more in extent; and (3) species which undertake migrations less in extent than 30° lat., or species which do not migrate at all. We may now consider each of these groups in turn, with regard to the question at issue.

I. *Species with protracted but irregular migrations.*

Diomedeidæ.
Procellariidæ.
Phaethontidæ (?)
Fregatidæ (?)

II. *Species with a migration range of 30° lat. or more.*

Podicipidæ.
Urinatoridæ (all ?)
Stercorariidæ.
Laridæ (most).
Sulidæ (most ?).
Anatidæ.
Gruidæ.
{ *Rallus virginianus.*
Porzana.
Fulica americana. }
Phalaropodidæ.
Recurvirostridæ.
Scolopacidæ (most).
Charadriidæ (most).
Aphrizidæ.
Columbidæ (most).
Cathartidæ (?).
{ *Circus hudsonius.*
Accipiter velox.
Falco (2 species). }
Ceryle alcyon.
Sphyrapicus varius.
Micropodidæ.
Trochilus colubris.
{ *Tyrannus* (2 species).
Myiarchus crinitus.
Sayornis (2 species).
Contopus (3 species).
Empidonax (5 species). }
{ *Dolichonyx oryzivorus.*
Icterus galbula. }
{ *Calcarius.*
Zonotrichia (2 species).
Spizella (2 species).
Melospiza (2 species).
Habia ludoviciana.
Passerina cyanea.
Spiza americana. }
Piranga (3 species).
Hirundinidæ.
Vireo (4 species).
Mniotiltidæ (most).
Motacillidæ (most).
Galeoscoptes.
Regulus (2 species).
Turdus (3 species).

Now the species enumerated in Lists I and II migrate periodically through an area of 30° lat., or more, that is, a migration range of considerable extent, and, with a few exceptions to be considered later, all are sharply defined species, and even though the breeding areas of most are very broad, none of them have a tendency to split into geographical varieties. Accordingly there must be be some relation existing between

the range of migration and the tendency to produce geographical races, for otherwise this coincidence could not be explained. So having found that those species undertaking long migrations do not, as a rule, tend to give rise to local varieties, we must conclude that the process of taking extensive migrations is a check upon the tendency to produce geographical varieties. But in order to round off further deductions, we must first determine whether species which do not migrate extensively have a greater tendency to geographical variation than those just considered; and this assumption will be strengthened by a comparison of the species in the following List III with those in Lists I and II.

III. *Species with a short or no migration range.*

Alcidæ.
Rhynchops.
Anhinga.
Phalacrocoracidæ (most).
Pelecanidæ.
Phœnicopterus.
Plataleidæ.
Ibididæ.
Ciconiidæ.
Ardeidæ (most).
Aramidæ.
Rallidæ (most).
Hæmatopodidæ.
Tetraonidæ.
Phasianidæ.
Cracidæ.
Falconidæ (most).
Strix pratincola.
Bubonidæ.
Psittacidæ.
Cuculidæ.
Trogonidæ.
Momotidæ.
Alcedinidæ (most).
Picidæ (most).
Caprimulgidæ (most).
Trochilidæ (most).
Cotingidæ.
Tyrannidæ (most).
Alaudidæ.
Corvidæ.
Icteridæ (most).
Fringillidæ (most).
{ *Euphonia.*
{ *Piranga* (most).
Ampelidæ.
Laniidæ.
Vireonidæ (most).
Cœrebidæ.
Mniotiltidæ (a few).
Cinclidæ.
Troglodytidæ (most).
Certhiidæ.
Paridæ.
Polioptila.
Turdidæ (most).

It is at once apparent that almost all the species of North American birds which are divisible into geographical varieties are classed in this third list, that is, that those species evincing the greatest tendency to geographical variation, are also those which undertake migrations of the least extent. Thus, for instance, *Melospiza fasciata* is usually resident in most localities throughout the whole year, and has become differentiated into a number of geographical races, while *Melospiza georgiana* is migratory, and though it breeds in an area nearly equal in extent to that of *fasciata*, has not produced local varieties; the non-migratory *Megascops asio* shows great geographical variation, while the migratory *Asio acciptrinus*, though almost cosmopolitan in its breeding area, shows no tendency toward such variation. And, in fact, an examination and comparison of List III with Lists I and II, will lead to the conclusion, that given any two species of equally extensive breeding areas, the one with the smaller range of periodic migration will, as a rule, evince a greater tendency to produce geographical varieties than will the species with the greater range of migration. This conclusion may be concisely formulated as follows; *it is the rule that the amount of geographical variation in species with more or less extensive breeding areas, stands in inverse ratio to the extent of its periodic migrations.* Naturally, this law is only applicable to species with extended breeding areas, since diverse conditions in different sections of this area are necessary, according to the theory of Natural Selection, for the production of geographical subspecies or varieties; and in a limited breeding area, throughout which the conditions of the environment are similar, there could be no cause to produce geographical varieties, irrespective of the migratory or non-migratory habits of the species.

I have not meant to imply, in the preceding pages, that species with migration ranges of 30° lat., or more, are all sharply definable, *i. e.*, that such species are never divisible into geographical varieties; but, on the contrary, that this tendency to produce geographical races is less in the species with extensive migrations, than in those with shorter ranges of migration. For it is usual, even in species with extensive migrations, whose

breeding areas are extraordinarily great, so as to include the whole of the arctic region, or northern America together with northern Eurasia, for them to subdivide into two geographical varieties, occupying respectively the eastern and western hemispheres. Thus, the eurasiatic *Colymbus nigricollis* is represented by a variety (*californicus*) in western North America; and to give other examples where an eurasiatic form, which undertakes long periodic migrations, is represented by a geographical variety in North America, may be mentioned one species of *Fratercula*, 1 *Uria*, 1 *Larus*, 1 *Hydrochelidon*, 2 *Aythya*, 1 *Glaucionetta*, 1 *Somateria*, 1 *Anser*, 1 *Tringa*, 1 *Limosa*, 1 *Charadrius*, 2 *Falco*, 1 *Pandion*, and others. But no species with extensive migration ranges shows any tendency to geographical variation, unless its breeding areas are also very large in extent. And the species with the least demonstrable tendency to produce local races, are those in which the wing power is greater, and the range of migration more extensive, than in any other species of birds, namely, those enumerated in List I. Further, we find it to be the rule, that in those avian families most of the species of which undertake long migrations, if species are present which are divisible into geographical varieties, that these latter are more restricted in their migrations than the former; examples are *Uria troile*, *Rissa tridactyla*, *Fulmarus glacialis* (only North American species of the family presenting geographical varieties), *Rallus longirostrus*, *Porzana jamaicensis*, *Aegialitis wilsonia* and *Ae. meloda*, and others. After the consideration of these facts it is certainly permissible to conclude that, as a rule, species which undertake annual migrations of comparatively great extent, through distances of 30° lat., or more, evince no tendency to give rise to geographical varieties, unless their breeding areas are very extensive; and, conversely, that species which do not undertake extensive migrations, owing to insufficient wing power or to some other cause, and which occupy broad breeding areas, have the tendency to produce geographical varieties. Consequently, also, extended migration acts as a check upon the production of varieties; and the extent of the range of migration will, therefore, stand in inverse ratio to the amount of geographical variation

evinced. Thus the postulate of Darwin, that wide-ranging species vary most, must be modified after a consideration of the facts given here. But to pass over to certain apparent exceptions to the rule. *Falco columbarius*, breeding chiefly north of the United States, and migrating in winter as far as South America, has a variety (*suckleyi*) on the Pacific coast from Sitka to California ; *Helminthophila ruficapilla*, breeding as far north as Hudson's Bay, and migrating in winter as far as Guatemala, has a variety (*gutturalis*) from the Rocky Mts. to the Pacific coast, in winter to Mexico ; and a number of similar cases could be mentioned, where the species, although it has a wide range of migration, and a breeding area which is not extraordinarily extensive, has, nevertheless, the tendency to geographical variation. But such apparent exceptions to the rule are, in fact, not valid objections, since in these cases the geographical variety is much more restricted in the range of its migration than the type species, or *vice versa*. And in any of these cases, the species, including the variety, is to be regarded as a number of individuals, some of which undertake extensive migrations, while others migrate not at all or through much shorter distances. Therefore, these are not true exceptions to the law, that the extent of the migration stands in inverse ratio to the amount of the tendency to produce geographical varieties; since a number of the individuals do not undertake extensive migrations. Real exceptions may, however, be found in such cases where the individuals of the type species as well as its varieties make prolonged periodic migrations ; and after a careful examination of all the North American species and their varieties, I have found only four species which represent such exceptions to the rule : *Dendroica æstiva*, with its variety *morcomii*, *Seiurus noveboracensis*, with the subspecies *notabilis*, *Sylvania pusilla*, with the variety *pileolata*, and *Turdus ustulatus*, with its eastern variety *swainsonii*. These four species represent cases where, with not very extensive breeding area, both races of the species possess extensive migration ranges. But I think that the importance of these cases as exceptions to the rule is diminished, when we consider that in each case the migration route of the variety is different from

that of the species, one being west of, while the other is east of the Rocky Mts. And hence, since not only in its breeding area but also in its migration range, the variety is subjected to conditions of environment different from those influencing the type species, we would naturally expect that the species (as a whole) would become differentiated into two geographical races.

The reason for the law, that extensive migration acts as a check upon the production of geographical varieties, is not far to seek. The barn swallow, for instance, remains in its breeding area from four to five months each year, spending the remainder of its time, except that consumed by its actual migration to and fro, in its tropical winter quarters. Roughly speaking, we may say that it spends about half a year in its breeding area, and the remainder in its winter home. In other words, the swallow is subjected to one environment for half the time of its existence, and to a more or less different environment during the remainder of its life. The result of this on the organism is obvious: the action of the two environments during approximately the same length of time, would prevent it from becoming more particularly adapted to the one than to the other, and would lead to the production of more generalized characters, fitted to respond more or less equally to both environments. In this way individuals of the species could not become especially adapted to a certain portion of the breeding area, if such adaptations should be unfavorable for its existence in the winter quarters, and *vice versa*; in other words, the influence of the winter environment acts as a check upon the acquisition of adaptations suited alone to the summer environment. This is, to my mind, the only adequate explanation for the law that extensive migration exerts a check upon the production of geographical varieties. Species with wide-ranging breeding areas, on the other hand, but with none or only restricted migrations, may give rise to geographical varieties, suited respectively to the diverse conditions found in different portions of its habitat, since such species are influenced by the conditions of but one environment, owing to the absence or restriction of migration.

11 Sept., 1895.
West Chester, Penna.

THE PLANT-GEOGRAPHY OF GERMANY.

By Roscoe Pound.

In a recently published address Dr. Coulter speaks of a new movement in botany which is sending botanists "to the great laboratory of nature," and replacing collecting trips by biological surveys. "The old-fashioned collection of plants," he says, "will hold no more relation to the new field work than the old geology, with its scattered collection of fossils, holds to the topographic geology of to-day." Geographical botany as it is now understood is comparatively a recent development. Collectors and cataloguers for a long time have been gathering a portion of the bare facts upon which geographical botany must proceed, and the facts of plant-distribution have been more or less ascertained. But the systematic collating and grouping of these facts and the application of biological and physiological facts to them is a matter of the last few years and is still going on. At first localities were catalogued, and collectors were eager to add new and rare stations to those recorded for species; then came statistical comparison of families and genera, especially in relation to altitude and the media of plant-migration. The limits of distribution of species were ascertained, particularly of those which are characteristic and controlling in vegetation. Such work laid the foundations of geographical botany.

But the statistics as to the distribution of families, which have been worked out in one method and another, gave no promise of leading to important results. It was not until biological groups began to be made for the purpose of comparison, and statistics began to be applied to those groups, that such work acquired importance. It is apparent that a mere statement of the number of species of the various natural plant-groups occurring in a certain region tells us very little of the vegetation of that region except in the most general way. A group represented by comparatively few species may yet as far as the occupation of the soil is concerned be dominant and

controlling. To understand the vegetation of a region one must ascertain not only what are its physical, meteorological and geological features, but much more what sorts of plants control its water, meadow, plain, or forest vegetation. Directed towards the latter ends, statistics have a very different meaning. Such work is the aim of the new geographical botany. "When we hear of a district," say Schrœter and Stebler, "that it is covered with extended fields of turf-rush or of brome-grass, that tells us more of the nature of the region than long lists of meteorological data. It also tells us more than the mere occurrence of the species in question of itself"

A notable contribution to this department of the science is Dr. Drude's new work, "Deutschlands Pflanzengeographie," of which the first part appeared in January last. The sub title of the work gives a clue to its purpose. It is stated to be "ein geographisches Charakterbild der Flora von Deutschland." Much has been done in recent years towards such characterization of restricted districts, or for large areas as regards certain kinds of vegetation. But Dr. Drude in giving a complete picture of the vegetation of as large a country as Germany has, in one sense, made an epoch in geographical botany. Such a work demonstrates that the era of preparation is passed. A mere cursory examination of the work serves to convince the reader that the theory and system of plant-geography have been thoroughly worked out, and that henceforth workers will be busied chiefly with their application to other regions rather than with devising new methods.

As has been remarked, in order to be of value, statistics must be based not upon the systematic groups of plants but upon groups founded on biological considerations, so far as they indicate a positive role in the vegetation of the region in question. Such groups are called vegetation-groups. Dr. Drude points out also that the proportions of the number of representatives of the several orders, genera, or other systematic groups are not to be reckoned with the whole flora of a region as represented by a certain number of species, but with the biological plant-community of the region. Accordingly he constructs some thirty-five vegetation-groups for the flora of

Germany. The thoroughness of this may be judged from the fact that he begins with trees and ends with plankton-algæ.

Germany belongs to the Middle-European region which, bounded by the Pyrenees, the Alps, and the Balkan system, stretches along the northwest border of the Russian steppes to the arctic flora which extends over the north of Europe. The region includes also the wooded portions of the Scandinavian countries. Throughout this large region, as regards the distribution of families and genera, the same fundamental character prevails. Carrying the principles of division further, and observing on the one hand lesser influences of climate and physiognomy and on the other the division of the floral-elements into "Genossenschaften," subdivisions, or "Vegetationsregionen" are made. Germany and the neighboring regions of the Alps and Carpathians fall into five such divisions; the region of the north-Atlantic lowlands, the region of the south-Baltic lowlands and uplands, the region of the middle and south German highlands and lower mountain districts, the region of the higher mountain districts and subalpine formations, and the region of the higher mountain formations of the Alps and Carpathians. The region of central France and the west-Pontic region, to which belong the southwestern and southeastern neighbors of Germany respectively, include also isolated spots in Germany itself. Dr. Drude's maps show that the first two regions are continuous in extent. The first includes Holland and North Germany west of the Elbe and the western portion of the Danish peninsula, the second East-Prussia and Pomerania, being bounded roughly by the Oder on the west. Between the Elbe and the Oder is a neutral zone, transitional between the two regions. The whole of middle and south Germany to the Alps constitutes the third region. But along the northern borders of the Alps and here and there throughout south Germany, as for instance the Harz forest, the Thuringian forest, the Black forest, in isolated spots, we find the fourth region, the region of subalpine forests. Along the upper Rhine here and there are localities belonging to the region of central France, and in the southeastern portion are many localities belonging to the west-Pontic region.

But geographical botany today does not stop with the distribution of the wild flora. Cultivated plants, native useful plants, weeds, and the flora of waste places come in for their share of consideration and are treated in turn. The plants whose seeds are mixed with those of cultivated plants and are thus sowed and grown involuntarily are placed in the group of cultivated plants. But a more important group is formed by the species introduced and supported incidentally by the cultivation and occupation of the soil by man. A notable instance of this is a group of "saltpetre plants" due to the use of nitrate fertilizers.

It would become tedious to enumerate the many striking features of the work and the ideas which they suggest. The work is in some sort a summary of geographical botany as it now stands. So much material necessarily takes on a new aspect when brought together and digested, though we have been more or less acquainted with a large part of it in its scattered condition. As part of a whole, each fact seems something new. We may safely predict that a great impetus will be given to this kind of botanical work in regions remote from Germany by Dr. Drude's book, since it presents a practical outline which will not fail to be taken advantage of. Our own country furnishes many excellent opportunities which the various biological and botanical surveys now in progress are already beginning to seize. The example of such a geographico-botanical survey of a large country, on á large scale, will be a great inspiration.

Dr. Drude's book is most interesting reading, and as a compendium of the latest results in a growing and important department, as well as in its more immediate purpose, is of the highest value.

EDITOR'S TABLE.

The bestiarians are still actively engaged in endeavoring to prevent humanitarians from prosecuting their good work of relieving human disease and suffering. Their latest move is to endeavor to get national legislation to suppress physiologic research by vivisection in the Dis-

trict of Columbia. There are various reasons why humanitarians should take especial pains to prevent this attempt to restrict human knowledge and prevent the dimunition of human suffering. They suppose, that National legislation once secured, State legislation will be easily obtained. Perhaps they expect to get a national law forbidding such research in all parts of the United States! Such people must, however, present very clean hands in the cause of prevention of cruelty to animals before they appear as advocates of the suppression of the most important method known of reducing human suffering. Do any of them wear articles made from the furs of animals? Do they carry pocket-books or grip-sacks made of the skins of animals? Do they permit animals to be plucked of feathers for their comfort or ornament? Finally, do they encourage the enormous slaughter of animals by land and sea, for food and other purposes?

There is much important work done in the departments at Washington which will be affected by the bill that is soon likely to come before the Senate, and the educational institutions of the highest grade will be injured by it if it passes.

The bill it is said will be favorably reported to the Senate. It will, however, probably not come up for final action before the next session. Meanwhile biologists and humanitarians generally should urge on their Senators and Representatives the importance of defeating the bill in the interest of progress and humanity. Let them write to their Representatives for the Public Documents on Antivivisection of the District Committee of the Senate. The Medical men are active, but the biologists are not yet sufficiently awake to the importance of the situation. If members of the National legislature are fully informed, they will hardly pass the bill.

RECENT LITERATURE.

The Cambridge Natural History.[1]—Sometime ago we referred to the volume of this series containing the Molluscs and Brachiopods; the second volume in order of publication is now before us. As in the former volume there is a great lack of uniformity in the different parts

[1] The Cambridge Natural History, Vol. V. Peripatus by Adam Sedgwick; Myriapods by F. G. Sinclair; Insects, Part I by David Sharp. London, Macmillan and Co., 1895, pp. xi–584.

which compose it, a lack, in part attributable to the individuality of the authors, in part to an apparent failure on the part of the editors to lay down guiding rules for their authors.

Mr. Sedgwick devotes 26 pages to Peripatus, giving a good general account of the group, in its structure, development and habits, and following it with a list of the known species, essentially the same as that in his previous monograph. From his familiarity with the group no one was better able to treat of the group than he.

Mr. Sinclair should have been almost equally familiar with the Myriapods for he has published both on the structure and the embryology of the group, and yet his account is much less satisfactory. The general account of the habits is good and is based to a large extent upon the author's own observations, but we wish he had put into English some of the facts ascertained by vom Rath. The classification adopted, that of Koch, is rather antiquated (1847) while the investigations of Grassi, to say nothing of the later researches of Schmidt and Kenyon, show that the Scolopendrellidæ and Pauropidæ are not to be set aside as distinct from the Diplopoda, and the elevation of Cermatia to ordinal rank has very little in its support. One or two typographical errors are annoying. Scudder's figures of fossil Myriapods are attributed to "Meek and Worth," the author persisting in depriving the American paleontologist of the last syllable of his name. Here may be mentioned one of the inequalities of the work. While in treating of Peripatus a diagnosis is given of all (?) known species, in the Myriapods only the families are thus treated. Concluding the account is a discussion of the relationships of the group, and in this we find mixed up myths from Pliny and facts from other authors, including (p. 78) a quotation showing that the people of Rhytium were driven from their quarters by Myriapods, a statement which also occurs (p. 30) in another place. But in this whole part we see nothing but a feeble groping, not the firmness of the master hand. The chapter as a whole shows the lack of editorial supervision ; its prolixity on minor points should have been suppressed.

The best of the book is that by Mr. Sharp—accounts of the Aptera, Orthoptera, Neuroptera and the lower Hymnoptera, the author using these names in the widest sense. In the introductory sections, dealing with the anatomy and embryology of the Hexapods, the author is evidently less at his ease that in the more systematic portion. Here he has given us one of the best of all books upon insects. The strictly systematic portion is well done, while the account of habits and transformation is excellent, and the perspective good. Thus the Mallophaga

are accorded 6 pages, the White Ants, 44. On the whole we like the retention of the almost Linnean system of classification, especially since the systems which are proposed in its place are open to almost as many objections as the older scheme; the remarks made upon this point seem to us especially appropriate.

The illustrations, of which there are some 370, are all fresh and are very well engraved. Some of them would, we think, look better in "half-tone," especially those dealing with anatomical and developmental points, but against this is the apparent inability of English printers to get good results from such plates, (witness several translations from the German where these half-tone illustrations, beautifully printed in the original, are extremely muddy). One more fault and we are done. The price charged for the work seems to us much too high.

W. Fraser Rae's biography of Richard Brinsley Sheridan, that remarkable man "who could rival Congreve in comedy and Pitt and Fox in eloquence" is announced by Messrs. Henry Holt & Co. It is to be in two volumes, and to include portraits and facsimile autographs of Sheridan and his famous contemporaries. Interesting documents written by the Prince of Wales, Sheridan, the Duke of Wellington, and the Marquis of Wellesley will be made public for the first time. The Introduction is by the Marquis of Dufferin and Ava, who is a great grandson of Sheridan.

Geological Biology.[1]—This treatise, in octavo form of 395 pages, is a study of organisms and their time-relations. The general laws of evolution are stated, and their formulation explained by detailed descriptions of characteristic examples. The examples are, for the most part, taken from the invertebrate forms. Mutability of species is illustrated by *Spirifer strictus* Martin, var. *S. loganii* Hall, the progressive evolution of class, ordinal, subordinal, etc., characters, by *Magellania flavescens*; the modification of generic characters is shown by the life-histories of Brachiopod families. The history of the Spirifers, a study of Cephalopods, and the evolution of the suture lines of Ammonoids, are each in turn used to demonstrate the fundamental laws of evolution. Throughout the book the author emphasizes the idea that these laws are best understood by a study of fossil forms.

The closing chapter sets forth the philosophy of evolution from the author's point of view. Beginning with the statement that "Evolu-

[1] Geological Biology. An Introduction to the Geological History of Organisms. By Henry Shaler Williams. New York, 1895. Henry Holt & Co.

tion is concerned with two distinct fields of human inquiry," he distinguishes them as follows:

"On the one hand, *evolution* is the name for *the natural order of unfolding of the characters of organic beings that have lived* on the earth; on the other hand, evolution is the name for our conception of *the mode of operation* of the *fundamental energy of the universe.* Thus it will be seen that the notion of God is as intimately involved in a discussion of evolution as is the notion of an organism." He sees in evolution the mode of creation of organic beings, a process that has been more or less continuous throughout geologic ages. "It is this continuation of the process of phenomenalizing that distinguishes the mode of creation in the organic realm from that in the lower realm of inorganic matter. Whatever is characteristic of organisms was not created at once, but has been unfolded by degrees, and there is no reason for supposing that the process is not still going on. Such expressions as 'effort,' 'growth force,' 'reactions,' etc., used in describing the phenomena of evolution, all express the notion of the preëxistence of some unphenomenal property, or power, or potency, which constitutes the cause of the particular characters which are acquired by organisms in the process of their evolution."

The tendency of organisms to vary is designated by the author as primarily a force acting from within, to which he gives the name "*intrinsic evolution.*" Differentiation of form and function are expressions of vitality, but these are modified by conditions of environment and natural selection.

A summary of the leading points in the work are thus given:

"The great facts attested by geology are that the grander and more radical divergences of structure were earliest attained; that, as time advanced, in each line intrinsic evolution has been confined to the acquirement of less and less important characters; such facts emphasize with overwhelming force the conclusion that the march of the evolution has been the expression of a general law of organic nature, in which events have occured in regular order, with a beginning, a normal order of succession, a limit to each stage, and in which the whole organic kingdom has been mutually correlated."

This book will prove instructive to the general reader, both on account of its facts and generalizations. The author, as a distinguished specialist in paleontology presents facts in an authoritative way, so that the reader may feel safe in his premises. The inferences made are obvious, so that while there is little exposition of efficient causes of evolution in the scientific sense, one can agree with the general conclusions.

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General Notes.

PETROGRAPHY.[1]

Malignite, a New Family of Rocks.—Lawson[2] uses the name malignite for a family of basic orthoclase rocks constituting an intrusive mass, possibly laccolitic, in the schists around Poohbah Lake, in the Rainy River district, Ontario. Three phases of the intrusive mass are recognized—a nepheline-pyroxene-malignite, a garnet-pyroxene-malignite and an amphibole-malignite. The constituents common to all phases are orthoclase, aegerine-augite and apatite. In the nepheline variety the nepheline occurs as patches in the orthoclase, or as micropegmatitic intergrowths with it. The orthoclase is in poikilitic relations with all the other minerals, surrounding them like the glass

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Bull. Dept. of Geol. Univ. of California, Vol. I, p, 337.

in a partially crystallized lava. It was evidently the last component to solidify. The composition of the rock is as follows:

SiO_2	Al_2O_3	Fe_2O_3	FeO	CaO	MgO	Na_2O	K_2O	H_2O	P_2O_5	Total
47.85	13.24	2.74	2.65	14.36	5.68	3.72	5.25	2.74	2.42	= 100.65

This composition is so similar to that of the Vesuvian leucitophyres, that the rock is regarded as the plutonic equivalent of these lavas. The low percentage of silica and the high lime percentage separate the rock from the eleolite syenites.

One form of the nepheline-malignite is panidiomorphic through the development of the orthoclase and all the other components in crystals. In the garnet-pyroxene phase of the rock the orthoclose is intergrown with albite in the form of phenocrsyts imbedded in a hypidiomorphic aggregate of aegerine-augite, melanite, biotite, titanite and apatite. The augite, melanite and biotite are allotriomorphic. They seem to have crystallized contemporaneously with each other, and with a part of the orthoclase. In the amphibole-malignite the distinguishing characteristic is the prevalence of a very strongly pleochroic amphibole, and the absence of any large quantity of aegerine. The augite that is present occurs intergrown with the amphibole. Melanite is wanting, otherwise this rock is very much like the mellanite-pyroxene malignite.

The author points out the fact that the great mineralogical differences observed in the three types of malignite, are accompanied by very slight differences in chemical composition. The three types are regarded as differentiation phases of the same rock mass.

Foliated Gabbros from the Alps.—Schäfer[3] gives an account of the olivine gabbro and its dynamically metamorphosed forms which constitute the rocks of the region in the vicinity of the Allalin glacier between the Zermatthal and the Saarthal in the Alps. The normal gabbro contains in its freshest forms much or little olivine. In its altered forms it consists of saussurite, amphibole, talc, actinolite and garnet. Ottrelite is often found enclosed in the talc and sometimes imbedded in the saussurite. In one of the granular varieties of the metamorphosed gabbro a blue amphibole is very abundant. It is intergrown in part with omphacite. The granular alteration forms of the gabbro pass gradually into foliated forms and through these into rocks called by the author "green schists." The schistose gabbros are mineralogically similar to the granular alteration phases of the rock, except that they contain in addition to the minerals named above a

[3] Neues Jahrb. f. Min., etc.. B.B., p. 91.

newly formed albite, zoisite and white mica. The final stage of the alteration is a zoisite-amphibole rock. The green schists are composed of ellipsoids of zoisite, feldspar and epidote imbedded in a schistose green amphibole clinochlor aggregate. Some of the schists are rich in garnets, and others are practically chlorite-schists. All are supposed to be derived from the gabbro.

In addition to the gabbros there are also in the region several exposures of serpentine whose contact with the green schists with which they are associated are always sharp. The original form of the rock is unknown, but it is supposed to have been a peridotite. Its most interesting feature is the possession of light yellow and brown crystals of some member of the humite family.

On the west side of the Matterhorn the author also found normal and olivine gabbros, both more or less altered. The former is cut by little veins of aplite. The peak of the Matterhorn is scarred by numerous fulgurites. Its rocks are fine grained green schists, some of which are like those described above, while others are dense and homogeneous in appearance. They consist of amphibole, clinochlor, zoisite, altered plagioclase, talc and alkali-mica. These rocks are defined as zoisite-amphibolites.

The Rocks of Glacier Bay, Alaska.—Cushing[4] gives a few additional notes on the petrography of the boulders and rocks of Glacier Bay, Alaska. The principal rocks of the region are diorites, altered argillites and limestones that are cut by dykes of igneous rocks. In addition to the diorites and quartz-diorites reported by Williams[5] from this vicinity, there are also in the region mica and actinolite-schists. The dyke rocks are mainly diabases. The author gives some additional information concerning the diorites and briefly describes the schists. The actinolite schists are aggregates of finely fibrous actinolite needles, in whose interpaces is a granular mixture of quartz and epidote and an occasional grain of plagioclase. The mica schists present no unusual features except that some of them are staurolitic.

Petrographical Notes.—As long ago as 1836 Thomson reported the occurrence of light yellowish-green rounded masses which he called huronite, imbedded porphyritically in a boulder of diabase from Drummond Island. Other occurrences of the same substance have been found by the Canadian geologist in diabase dykes cutting the rocks of the Lake Huron region. These have been investigated by Barlow[6]

[4] Trans. N. Y. Acad. Sci., Vol. XV, p. 24.

[5] Cf. American Naturalist, 1892, p. 698.

[6] Ottawa Naturalist, Vol. IX, p. 25.

and are pronounced by him to be aggregates of zoisite, epidote, sericite and chlorite in a mass of basic plagioclase. In other words, huronite is a saussuritized plagioclase. Descriptions of a number of dyke rocks containing 'huronite' are given by this author.

Bauer[7] describes a number of specimens of snow-white, lilac and emerald-green jadeite from Thibet and upper Burmah. One of the green varieties is cut by little veins of nepheline, containing plates of basic plagioclase and little bundles of a monoclinic augite (jadeite) with the same properities as that which constitutes the mass of the jadeite. The rock, according to the author, is made up of this augite and nepheline, the latter mineral acting as a groundmass. The veins are those portions of the rock in which the augite is in very small quantity. In other specimens nepheline occurs in small quantity, and plagioclase is abundant. His conclusion is that the rock is a jadeite-plagioclose-nepheline rock in which locally the one or the other component is most prominent. If the rock is, as the author supposes, a crystalline schist, the occurrence of nepheline in it is of extreme interest.

In a second article the same author[8] describes a serpentine from the jadeite mines at Tauman. It is composed of olivine, picrolite, chrysotite, webskyite and a few other accessories in an albite-hornblende matrix, consisting of an aggregate of single individuals of untwinned albite, in the midst of which lie brown and gray hornblendes surrounded by zones of a bright green variety of the same mineral. Between this zone and the albite there is a fringe of green augite needles. The rocks associated with the jadeite and the serpentine are also described. Among them is a glaucophane-hornblende-schist. All the rocks exhibit the effects of pressure.

In a very short note Beck[9] calls attention to the fact that the molecular volume of dynamically metamorphosed rocks, i. e., of the minerals composing these rocks—is less than that of the original rocks from which they are derived. For instance, a mixture of plagioclase, orthoclase and water in the proportion to form albite, zoisite, muscovite and quartz has a molecular volume of 547.1, while the corresponding mixture of albite, zoisite, etc., has a volume of 462.5,

[7] Neues Jahrb. f. Min., etc., 1896, I, p. 85.

[8] Record of Geol. Survey of India, XXVIII, 3, 1895, p. 91.

[9] Kais. Ak. Wiss. in Wien. Math. Naturw. Class, Jan., 1896.

GEOLOGY AND PALEONTOLOGY.

Phylogeny of the Dipnoi.—In a memoir recently published, M. Dollo adduces fresh evidence for the theory recently advanced by various English scientists that the diphycercy among the Dipnoi is in reality a secondary diphycercy or gephyrocercy. The author regards this as an important fact, and uses it as a basis in developing his theory of the origin and evolution of the order. The results of his researches are as follows:

I. *Dipterus valenciennesii* is the most primitive of the Dipnoi known.

II. In a general way, the evolution of Dipnoi, since the lower Devonian, is represented in the following series in the order of the enumeration of its terms.

Dipterus valenciennesii—Dipterus macropterus—Scaumenacia—Phaneropleuron — Uronemus — Ctenodus— Ceratodus—Protopterus—Lepidosiren.

III. The origin of the Dipnoi must be looked for among the Crossopterygia.

IV. The Batrachians are not in the line of the Dipnoi.

V. The specialization of the Dipnoi has been from a pisciform type toward an anguilliform type.

This order is a terminal group, derived from the stem that gave origin to the Batrachians.

In conclusion the author gives the phylogeny of the gnathostome vertebrates in a tabulated form. (Bull. Soc. Belge de Geol. T. ix, Fasc. 1, 1895, Bruxelles, 1896).

Fauna of the Knoxville Beds.[1]—The Knoxville beds are a Cretaceous series confined to the coast ranges of California, Oregon and Washington. They are characterized by the great abundance of Aucella, usually without associates, but, through the explorations of Mr. Diller and other geologists, a rich and varied invertebrate fauna has been discovered in the Aucella-bearing series of the Pacific States.

The description of this fauna was assigned to T. W. Stanton, and is now published as Bulletin No. 133 of the U. S. Geological Survey. The author recognizes 77 distinct species and varieties, of which 50 are new. All but 7 of the species are Mollusca, including 33 species of

[1] Bulletin United States Geological Survey, No. 113. Contributions to the Cretaceous Paleontology of the Pacific Coast; The Fauna of the Knoxville Beds. By T. W. Stanton. Washington, 1895, [issued Feb. 3, 1896].

Pelecypoda, 1 of Scaphopoda, 18 of Gastropoda, 18 of Cephalopoda (15 Ammonoids, 3 Belemnites). The other 7 species include 5 Brachiopoda and 2 Echinodermata.

The brief introduction comprises a geological description of the beds, with a discussion of their age, and of their relations to various other formations characterized by a similar fauna.

The new species are figured on twenty page plates.

Notes on the Fossil Mammalia of Europe, IV.—On the Pseudoequines of the Upper Eocene of France.—Under the term Pseudoequines may be included the various species of the genus *Paloplotherium*, which occur in the Upper Eocene and Oligocene of Europe. This phylum parallels in a remarkable manner many of the characters which are typical of the true horses, but these characters, strange to say, are much earlier differentiated than in the real equine phylum.

Kowalevsky[2] in his great work on "*Anthracotherium*" clearly recognizes in his phylogenetic table of the Ungulates, that *Paloplotherium* is not in the direct line of the horses. Schlosser is also of the same opinion as Kowalevsky in regard to the relations of *Paloplotherium* to the horses. Professor Gaudry[3] as late as 1888 placed all the species of *Paloplotherium* in the direct line leading to *Equus*.

The earliest known species referred to *Paloplotherium* is the *P. codiciense* Gaudry; this form is from the Calcaire Grossier or Middle Eocene. In *P. codiciense* there are four upper and lower premolars, whereas in the more typical species of *Paloplotherium* from later deposits there are only three premolars. Moreover, in the *P. codiciense* all the upper premolars are simpler in structure than the true molars. The last upper premolar in this species is tritubucular in structure, and there are two well defined crests running outwards from the deuterocone, in other words this tooth is well adapted for further evolution into the molariform last premolar of the typical Paloplotheroids. In the true molars of *P. codiciense* the ectoloph has nearly the same form as that of the Palaeotheridæ in general, the metaloph or posterior crest, however, is less oblique in position than in the later species of *Paloplotherium* and *Palæotherium*. The type specimen of *Paloplotherium codiciense* consists of a facial portion of a skull with the teeth well preserved. This species is much larger than *P. minus* and corresponds more nearly in size with *P. annectens*.

[2] Monographie der Gattung Anthracotherium, p. 152.

[3] Les Ancêtres de nos Animaux, Paris, 1888.

In "La Petite Galerie" of Paleontology at the Jardin des Plantes, Paris, there is a well preserved skeleton of *Paloplotherium* which is labelled *Paloplotherium minus.* This specimen is of some importance, as we have here a case where the skeletal parts and teeth are associated and from the same individual. This skeleton was found in the Calcaire Grossier de Dampleix, Départment of Aisne,[4] and it is important to add that Professor Gaudry's type of *P. codiciense* is from the same horizon of this Départment, but from another locality.

Although this skeleton has been determined as *P. minus* it differs widely from this species, and also in fact from *P. codiciense.* In the upper and lower jaw there are four premolars as in *P. codiciense*, but these teeth are much more complicated than in that species and are exactly transitional in structure between *P. codiciense* and the typical species of *Palæotherium.* The premolars in this type are badly worn, but I can distinctly trace on one side that the internal cones are nearly double, but not distinctly separated as in *Palæotherium;* the structure the true molars is like that of *P. codiciense.* As will be seen the dentition of the skeleton above referred to differs from *Paloplotherium minus* in having four premolars, also the second upper tooth of this series is more complex than in the latter.

The parts of the skeleton associated with the teeth and skull consist of a scapula, radius and ulna, and also some metapodials. Among the latter there is a Mc. III and also another metacarpal, which I think may be Mc. V. If this determination be correct, we have here a Palæotheroid with four digits to the mamus. In the species of *Palæorium* from the Upper Eocene, Mc. V is represented only by a rudiment. I would like to add that the metapodial in this skeleton which I have determined as Mc. III is flatter and less triangular in section than in the typical *Paloplotherium minus;* this goes to show that the lateral toes were larger, and supports my view as to the presence of four anterior digits in this as yet undescribed species.

From the characters above adduced I conclude that this new species of Palæotheroid is more closely related to the true *Palæotherium* than to *Paloplotherium*, and moreover it is the most primitive form of *Palæotherium* yet discovered.

I am not able to learn that the beds in which this skeleton was discovered are any later than those in which the *P. codiciense* was found. However, from the structure of the premolars in the two species, I would conclude that *P. codiciense* came from an earlier subdivision of

[4] I am much indebted to my friend M. Marcellin Boule of the Jardin des Plantes for having given me information in relation to this specimen.

the Calcaire Grossier of Aisne than that in which this skeleton was found.

The most abundant species of *Paloplotherium* found in France is the *P. minus*. This species was described by Cuvier and referred to the genus *Palæotherium*, but it was later raised to a generic rank by Owen, and also by Pomel. In regard to *Paloplotherium minus* it is of importance to attempt to show that the teeth and feet of this species are properly associated. Osborn and Wortman[5] have lately questioned the correctness of this association, and furthermore these authors think it probable that the feet referred to *P. minus* by the French Paleontologists really belong to a small species of Lophiodont-like animal, closely related to the American genus *Colodon*. I cannot agree at all with these authors in this supposition, as I believe that the feet tending to monodactylism found in the Upper Eocene of France, which are referred by the French Paleontologist to *Paloplotherium*, are correctly identified.

Among the large collection of fossils in the Jardin des Plantes, many of which formed the types of Cuvier, and which were described by him in his "Ossemenes Fossiles," there is a nearly complete skeleton referred by Cuvier to *Paloplotherium minus;* this is figured by Cuvier[6] and also by Blainville.[7] In this specimen the feet are absent, but there are a few teeth embedded in the skeleton which have the same structure and size as those referred to *P. minus*. Again, Blainville figures an anterior extremity of a small Perissodactyles which he refers to *Paloplotherium minus*, and this specimen is of the same size as the fore limb of the nearly complete skeleton of *P. minus* described by Cuvier. Both these specimens are from the Gypse de Paris. However, since the time of Cuvier, *Paloplotherium minus* has been found in great abundance in the Upper Eocene of Débruge. The collection in the Jardin des Plantes from Débruge contains a large number of jaws and teeth, and portions of limbs containing numerous metapodials. These bones correspond exactly in size with those of the original skeleton described by Cuvier, and I am of the opinion that this is pretty conclusive evidence that the skeletal parts of *Paloplotherium minus* and the teeth are correctly associated. Moreover, I am not aware that any small Lophiodont Perissodactyle occurs in the Débruge Eocene. I use the term "Lophiodont" strictly in the sense as applied by Osborn and Wortman.

[5] Bulletin American Museum Natural History, 1895, p. 361.

[6] Ossemenes Fossiles, plate 115.

[7] Osteographie, Blainville, Palæotherium, plate VI.

Having now attempted to show that the monodactyle type of foot found in the Upper Eocene of France is in all probability correctly associated with the teeth of *Paloplotherium,* I shall review the characters of this phylum and indicate those points which parallel the true horses, and also point out those aberrant structures of the teeth which exclude the possibility of placing this series in the direct line leading to *Equus.*

Through the kindness of Professor Albert Gaudry, I have been enabled to study a beautifully preserved skull of *Paloplotherium javvalii* from the Phosphorites. This cranium is remarkably like that of the horse in many of its characters, and I think most Paleontologists would say at once that this type of horse-like skull should be associated with a foot tending to monodactylism. The position of the orbit is as in the primitive horses, its anterior termination being placed over the second true molar. The form of the facial region closely resembles that of the horse, being high and strongly compressed. The premaxillaries are elongated and slender, and slope gradually backwards as in the horse. Among the Palæotheroids, *P. crassum* has a skull resembling somewhat that of *Paloplotherium javalii,* but in the former the facial region is shorter and broader than in *P. javalii.* In the skull of *P. javalii,* there is a large flat area between the orbits, and the sagital crest is well marked. The post-orbital processes of the frontals are largely developed and extends well downwards towards the zygomatic arch. The post-tympanic and paroccipital processes are united as in *Palæotherium crassum.* The basal region of the skull in *P. javalii* is long and narrow, like that of the horse.

The structure of the skull in *Paloplotherium minus* is not known, only fragments of the occiput having been found. The teeth of *P. javalii* have been described by M. Filhol, and as is well known the crowns of the upper molars are much elongated and tending strongly to the hypsodont condition of *Equus.* Moreover, the valleys between the crests are filled with cements and the external and internal surfaces of the crown are coated with the same substance.

In *Paloplotherium* the last upper premolar is completely molariform and the posterior crest of this tooth, and that of the true molars is very oblique in position. The metaloph owing to its oblique position, only unites with the ectoloph after a long period of wear; this crest, in the true horses, moreover, is nearly at right angles to the ectoloph and unites early with the latter. In *Paloplotherium* also, the hypostyle—an element so essential in the evolution of the horse's molar, is absent. The lower true molars in the Paloplotheroids lack the reduplication of

the metaconid, which is present in the members of the true Equine phylum.

No metapodials of *P. javalii* have been found associated with the teeth ; however there are in the collection of the Jardin des Plantes and also in the École des Mines, a number of enlarged third metacarpals and metatarsals from the same beds in which they find the teeth of *P. Javali*, and in all probability belong to this species. As already stated the horse-like skull and teeth of *Paloplotherium javalii* support the view that this type of cranium belong with these specialized metapodials. The third metacarpal in *P. javalii* is long and slender, and has a large facet for the unciform, the section of this bone is triangular with the lateral surfaces very oblique. This structure of the metapodial shows that the lateral digits were placed far to the inside and behind. The posterior cannon bone is more progressive in its horse-like character than the anterior, the proximal surface is much expanded transversely and the postero-lateral cavities for the metapodials are placed further behind than in the fore foot.

M. Filhol has described remains of *Paloplotherium minus* from the Oligocene of Ronzon, and in these beds they again find the enlarged third metapodials which are so abundant in the Débruge Eocene. This is another proof that the teeth and podial elements in *Paloplotherium* are properly referred.

A form closely related to the Palæotheridæ is the genus *Anchilophus*. This genus is more normal in its tooth structure in comparison with the early horses than *Paloplotherium*, and is considered by some authors[8] as in the direct line leading to *Equus*. Kowalevsky[9] however, calls *Anchilophus* a "Versuchgenus in der Pferderichtung, der Versuch war aber erfolglos, und der Anchilophus erlischt im Eocän, ohne directe Nachfolger zu hinterlassen." Kowalevsky reached this conclusion from studying the carpal bones of *Anchilophus*. I have had access only to the teeth of *Anchilophus desmarestii* and consequently must base my conclusions upon the characters of one species only of this genus. A comparison of the superior molars of *A. desmarestii* with those of *Mesohippus*, a genus which is considered by all competent authorities to be in the true Equine series, shows the following differences: The ectoloph in both genera has nearly the same form, but in *Anchilophus* the mesostyle is absent, this is well developed in *Mesohippus*. In *A. desmarestii* the hypostyle is wanting, which is so prominent in the molars of *Mesohippus*. The direction of the metaloph in *An-*

[8] Etudes sur l'Historie Palaeontologique des Ongules. Par Mme. Pavlow. Bull. Soc. Nat. de Moscou, 1888, p. 148.

chilophus is less oblique than in *Paloplotherium* and is more as in *Mesohippus.* In the lower molars of *Anchilophus* the metaconid is not reduplicated, and the crescents are in form more like *Palaeotherium.*

From the fact that Kowalevsky, in studying the podial elements of *Anchilophus*, concluded that this genus could not be in the true Equine series, adds mnch weight to the view of its non-persistence. Again, we have seen that the molars of *Anchilophus* are wanting in a number of important elements which are present in all later genera leading to *Equus.* The above evidence points to the fact that *Anchilophus* must be considered as another aberrant form not leading to permanent results.—Charles Earle.

Reclamation of Deserts.—The shifting of the sand dunes in the Sahara desert frequently ends in destruction of fertile oases. To prevent the encroachment of the dunes upon the arable land has long been a problem with the French. Commandant Godron has inaugurated a system of tree planting in the neighborhood of Aïu-Sef-ra, Ouargla and El-Golea from which excellent results have been obtained. Following out the theory that tree plantations would prevent the dunes being at the mercy of the wind, and finally make them stationary, M. Godron planted a neighboring dune with seedlings of various species of trees and shrubs. To prevent the sand from shifting while the new plants were establishing themselves, a light covering of alfa straw was spread over the ground. This was found to effectually shield the sand from the action of the wind.

In making a plantation, Mr. Godron combines seeding, cuttings and plants already rooted. The species best adapted for growing on the dunes have proved to be the Barbary fig, peach, aspen, Italian poplar, weeping willow, *driun*, grape-vine, Spanish broom, acacia and roses.

To supply the demand for cuttings and rooted plants for this new desert industry, M. Godron has established local nurseries at Aïu-Sefra and at El-Golea. The water supply for maintaining the growth of vegetation is from artesian wells. The reclaimation of vast extents of desert land is hoped for in the future, through the adoption of the plantation methods of Commandant Godron. (Revue Scientif., Fev., 1896).

[9] Anthracotherium, p. 157.

BOTANY.[1]

Botany in the National Education Association.—An effort is now under way to bring about greater interest in the *teaching* of Botany than has hitherto been shown by American botanists. The new department of Natural Science Instruction is intended to bring together the teachers of science (Botany, Zoology, Chemistry, Physics, etc.) who are interested in science *as a means of culture*, and to stimulate thought and discussion as to how this end may best be obtained. What rôle should Botany play in the mental development of a man? In what way may the study of plants be made an efficient factor in a man's mental training? When and how should plant study be made a part of a man's training? These are some of the questions which will be discussed by the botanists in the Buffalo meeting of the National Educational Association on July 9th and 10th next. It is to be hoped that many who are interested in this department of Botany will be present.

—CHARLES E. BESSEY.

Coulter's Revision of N. A. Cactaceæ.—Nearly two years ago Dr. Coulter brought out the first part of his revision of the N. A. Cactaceæ (Contrib. U. S. Nat. Hist., Vol. III, No. 2), and now in No. 7 of the same volume we have the concluding part. The family as revised now includes North American genera and species as follows: *Cactus* Linn., Sp. Pl., 466 (= *Mamillaria* Haw. Synop., 177), with 64 species and varieties; *Anhalonium* Lem., Cact. Gen. Nov., with 5 species; *Lophophora* Coulter, a new genus, with 2 species; *Echinocactus* Link & Otto, Verh. Preuss. Gartenb. Ver., 3,420, with 52 species and varieties; *Cereus* Mill. Gard. Dict. Ed. 8, with 82 species and varieties; *Opuntia* Mill. Gard. Dict. Ed. 7, with 101 species and varieties. We have thus a total of 306 species and varieties of North American cactuses. The work is styled a *preliminary* revision, and the author says, in his prefatory note, that on account of the peculiar difficulties attending the revision "the undertaking would have been abandoned only that it seemed but proper to contribute to the knowledge of the group such facts as had come to light in the course of several years' study," a most commendable conclusion, indeed.

—CHARLES E. BESSEY.

Botanical News.—Dr. Charles A. White has recently prepared a Memoir of George Engelmann for the National Academy of Scien-

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

ces. It is a sympathetic sketch of the life of a strong and industrious man.

A recent bulletin (No. 10) from the Division of Forestry of the U. S. Department of Agriculture, with the title "Timber," contains much of general botanical interest. Such topics as "wood of coniferous trees," "wood of broad-leaved trees," "weight of wood," "shrinkage of wood," "mechanical properties of wood," etc., illustrate the scope of the work. It is deserving of a place in any botanical library

Professor R. A. Harper's confirmation of the act of fertilization in *Sphaerotheca costagnei*, in the *Berichte der Deutschen Botanischen Gesellschaft* (Bd. XIII, heft. 10) brings grateful relief from the monotonous repetition of doubts as to the accuracy of DeBary's work. The applicability of modern imbedding processes to the study of the life-history of the smaller fungi has rarely been better demonstrated than in this satisfactory paper.

The Eli Lilly Co., of Indianapolis, have recently issued an "Exchange List" of their herbarium. It includes 976 names, all in the modern nomenclature.

The Report of the Botanical Department of the New Jersey Experimental Station, by Dr. Halsted, possesses more than the usual interest of similar publications. With much of practical value, the author has mingled a great deal which possesses high scientific interest.

In Professor Scribner's New North American Grasses (*Bot. Gaz.*, March, 1896), four new species and one new genus are described, viz.: *Avena mortoniana* from Silver Plume, Colo.; *Danthonia parryi* from Georgetown, Colo.; *Zeugites smilacifolia*, a curious broad-leaved species from Cuernavaca, Mexico, and *Pringleochloa stolonifera*, from the vicinity of Mt. Orizaba, Mexico. The new genus is apparently very close to *Bulbilis* (*Buchloë*).

The Field Columbian Museum recently issued No. 2 of the Botanical Series of its publications. It contains the "Flora of West Virginia," by Dr. Millspaugh, and is a considerable enlargement of an experiment station report made by the same author a couple of years ago. It includes 2584 names, of which 980 are Fungi, 115 Lichens, 123 Bryophytes, 57 Pteridophytes, and 1309 Authophytes. The nomenclature is modern and the work is well done, but one is sorely puzzled with the peculiar sequence of families in the Fungi, in which one finds in strange juxtaposition *Saccharomycetaceæ*, *Diatomaceæ* and *Myxomyceteæ* (pp. 84–85).

Professor Greene's "New Western Plants," in the Proceedings of the Academy of Natural Sciences of Philadelphia (Feb. 7, 1896), con-

tains descriptions of some interesting plants, e.g., *Trifolium truncatum*, *T. lilacinum*, *T. rostratum*, *Boisduvalia diffusa*, *Valerianella magna*, *V. ciliosa*, *Lessingia pectinata*, *Pyrrocoma eriopoda*, *P. solidaginea*, *P. subviscosa*, *Aster militaris*, *A. frondeus* and *Vagnera pallescens*. To the same paper is appended a revision of the genus *Tropidocarpum*, including four species.

An important paper comes to us from the College of Agriculture of the Imperial University of Japan (Bull. 5, Vol. II, Dec., 1895). It includes a descriptive list of the winter state of the trees of Japan, by H. Shirasawa, illustrated by twelve crowded plates of twigs and buds.

Dr. J. C. Arthur has found out that the common notion of farmers that one of the seeds in the bur of the Cocklebur (*Xanthium canadense*) germinates one year and the other does not grow until the following or some subsequent year is true. He details his observations and experiments in a paper in the Proceedings of the 16th Annual Meeting of the Society for the Promotion of Agricultural Science. "The purpose of this seemingly unique character is to distribute the two seeds of the bur in time, the customary distribution in space being impossible owing to the indehiscent structure."

The announcement that the *Botanical Gazette* is hereafter to be published by the University of Chicago will please every friend of science, since it insures its permanence and provides for that growth which the development of American botany demands.

Suggestions About Antidromy and Didromy.—The interesting notes from Prof. Todd in the NATURALIST for March seem to me to bear upon a phenomenon which is different from what I have called antidromy: upon the *secondary changes* in the ordinary growth which seem to be intimately related to the direction of light and the necessities of exposure to air. They are commonly shown by the foliage of such plants as the elm, morningglory, peach, and Forsythia, and very often by flowers, in which they may subserve cross-fertilization.

Antidromy, in its strict sense, is a diversity of a primitive character, arising phylogenetically away down in the cryptogams, and maintained with singular constancy through all the Phaenogams; and ontogenetically it starts in the ovule, depending on the circumstance that every plant bears two castes of seeds, one set on each border of the carpellary phyllome. I have not yet determined whether it is a dextrose seed that grows on the right margin of the carpel: but all the carpels on the same plant seem to retain the twist of the plant which bears them (well

shown by the twisted pods of species of *Prosopis*), whilst the seeds which arise from the two sides of a carpel are diverse. The order of development in the carpel may depend on the direction of the nutriment, and on the lines of least resistance in the crowded condition of young organs resulting in right-handed and left-handed ovules. The outcome is that in the adult plant the whole phyllotaxy, including the floral structure and ramification of the entire organism and its inflorescence, are of one and the same order in any one individual plant, and of a different order in other plants of the same species. Whether this is of any special advantage to the grown plants I cannot say; but possibly by imparting different habits of growth to the various members of crowded vegetation, it may cause them to separate from each other, and so may diminish the intricate interlacing which is so injurious to gregarious plants.

Now that the season of vegetation is returning it is to be hoped that some of our young botanists will make and record their observations on this subject. We want especially to find out exceptions, apparent or real. My first paper was incorrect as to the supposed antidromy of rows of grains in the case of maize; dissection seemed to teach this; but I might have foreseen that the ear is just like the male panicle, having a disorderly crowd of grains rearranged by a secondary process into orderly rows, each row, however, including both dextral and sinistral grains. The case of the Bilsted (*Liquidambar*) is a puzzle, some of the branches of the same tree having dextral phyllotaxy, and others having sinistral phyllotaxy; this is the only case of the true internal antidromy known to me, though I shall not be surprised by the discovery of other similar cases. A somewhat similar condition is reported to me by Prof. Francis E. Lloyd, of Forrest Grove, Oregon, in ten cases of *Acer circinatum*. Perhaps these cases are allied to that of plants arising from rootstalks, as *Iris* and *Calla*, *Helonias*, *Nuphar*, etc., in which different plants arising from the same rootstalk are antidromic. It will be worthy of examination whether sarmentose plants, as strawberry, and the *Saxifraga* described by Prof. Todd are antidromic as between those grown from the same original stock.

The phenomenon which I have termed *didromy*, where the same member is twisted in opposite directions at its two extremities, seems to me to be always related to the immediate life of the plant, and to have no genetic significancy. The didromic twist of the awn of *Danthonia* and other grasses, results in the upper part penetrating an object so soon as the lower part untwists by the application of water: that of the long peduncle of *Vallisneria* approximates the extremities, thus

pulling the fertilized flower down through the water without turning it around. If I am correct in the observation that some plants of Vallisneria have the dextral twist at the lower part, and others have the sinistral twist below, this would be a complex of the primitive antidromy having superposed upon it a recently acquired didromy.

A different line of investigation will search out the relation of dextrorse or sinistrorse phyllotaxy to the leaf-traces in the stem. I am convinced that inattention to this point has marred some of the work on the histology and the plan of the fibrovascular bundles, and that even with opposite-leaved plants, many species will be found to exhibit a duplicate pattern as between the arrangements in different individuals of a species.—George Macloskie.

Princeton College, March 16, 1896.

VEGETABLE PHYSIOLOGY.[1]

A New Classification of Bacteria.—In a recent number of *Die Natürlichen Pflanzenfamilien* (Lieferung 129, Leipsic, 1896) Prof. W. Migula, of Karlsruhe, gives a classification of the bacteria which is much more practical and satisfactory than that of Dr. Alfred Fischer, noticed in the September (1895) number of this journal. Migula's arrangement seems, on the whole, to be the best yet devised, and will probably come into general use, at least among botanists. The characters of several genera are amended, properly it seems to the writer, e. g., *Bacterium, Bacillus, Streptothrix,* and other genera are discarded as being founded on purely biological grounds, e. g., *Photobacterium, Nitromonas, Clostridium.* Of course, biological peculiarities are recognized as indispensable in the differentiation of species. In reading this paper one is occasionally surprised at the omissions, but taken in its entirety the work of consulting literature seems to have been very carefully done, and what is more important the classification appears to have grown out of a long and wide experience in the laboratory, and seems to be eminently usable. This paper treats briefly of most important literature, morphology, vegetative condition, resting state, cultures on artificial media, biological peculiarities, geographical distribution, rela-

[1] This department is edited by Erwin F. Smith, Department of Agriculture, Washington, D. C.

tionships, earlier classifications, etc., and then proceeds to the description of the families and genera. The group Schizomycetes forms the first section of the Schizophyta, and is divided into five families; Coccaceæ, Bacteriaceæ, Spirillaceæ, Chlamydobacteriaceæ and Beggiatoaceæ, as follows:

I. Cells globose in a free state, not elongating in any direction before division into 1, 2 or 3 planes — 1. Coccaceæ.
II. Cells cylindrical, longer or shorter, only dividing in one plane, and elongating to twice the normal length before the division.
  (1) Cells straight, rod-shaped, without a sheath, non-motile or motile by means of flagella — 2. Bacteriaceæ.
  (2) Cells crooked, without a sheath — 3. Spirillaceæ.
  (3) Cells enclosed in a sheath — 4. Chlamydobacteriaceæ.
  (4) Cells destitute of a sheath, united into threads, motile by means of an undulating membrane — 5. Beggiatoaceæ.

The genera recognized by Prof. Migula are as follows:

(1) Coccaceæ
  A. Cells without organs of motion
    a. Division in one plane — 1. Streptococcus.
    b. Division in two planes — 2. Micrococcus.
    c. Division in three planes — 3. Sarcina.
  B. Cells with organs of motion
    a. Division in two planes — 4. Planococcus.
    b. Division in three planes — 5. Planosarcina.
(2) Bacteriaceæ
  A. Cells without organs of motion — 1. Bacterium.
  B. Cells with organs of motion (flagella)
    a. Flagella distributed over the whole body — 2. Bacillus.
    b. Flagella polar — 3. Pseudomonas.
(3) Spirillaceæ
  A. Cells rigid, not snake-like flexuous
    a. Cells without organs of motion — 1. Spirosoma.
    b. Cells with organs of motion (flagella)
      1. Cells with 1, very rarely 2–3 polar flagella — 2. Microspira.

2. Cells with polar flagella-tufts 3. Spirillum.

B. Cells flexuous 4. Spirochæta.

(4) Chlamydobacteriaceæ

A. Cell contents without granules of sulfur

a. Cell threads unbranched

I. Cell division always only in one plane 1. Streptothrix.

II. Cell division in three planes previous to the formation of conidia

1. Cells surrounded by a very delicate, scarcely visible sheath (marine) 2. Phragmidiothrix.

2. Sheath clearly visible (in fresh water) 3. Crenothrix.

b. Cell threads branched 4. Cladothrix.

B. Cell contents containing sulfur granules 5. Thiothrix.

(5) Beggiatoaceæ

Only one genus known (Beggiatoa Trev.) which is scarcely separable from *Oscillaria*. Character as given under the family.

This scheme is simple in comparison with that of Fischer, but to fully appreciate it one should compare it with that of de Toni and Trevisan in Saccardo's *Sylloge Fungorum*, where cumbrousness and triviality reach a climax, these authors breaking up the group into no less than 50 genera.

From among the various general statements we cull the following: For the most part the wall of the cell is formed not out of cellulose or any similar carbohydrate, but out of albuminoids. The wall may, however, contain embedded in its substance variable quantities of a carbohydrate coloring blue with iodine. There is no "centralkörper" in the true bacteria, but vacuoles have frequently been mistaken for such bodies. Most bacterial pigments are non-nitrogenous bodies related to the analin colors; others are nitrogeneous substances related to the albuminoids, such apparently are the fluorescent pigments. The manner of cell division is a fundamental distinction between Bacteriaceæ and Coccaceæ. Cell division always takes place at right angles to the longitudinal axis when any such is clearly visible. With few exceptions motility is accomplished by means of flagella. These are

extremely delicate protoplasmic structures originating directly from the membrane. This mode of origin makes it still more probable that the wall of the cell is only an external denser layer of plasm. Bacteria in which the contents has been drawn away from the point of insertion of the flagella by plasmolysis are apparently still capable of motion. The flagella are either fastened to one or both poles of the cell or else are scattered irregularly over the whole body. This position of the flagella on the bacterial body is constant and may be used to distinguish genera. In some species all of the plasma of the mother cell is not used up in spore formation, but sometimes a considerable part is left in the rod. In germination the spores swell up, lose their refractive power, and usually open either at the pole or equatorially allowing the young germ to protrude, but sometimes the wall of the spore entirely deliquesces before the germ has protruded, the latter simply elongating as a vegetative cell. It is not always easy to determine the character of the refractive contents of bacterial cells, and in such cases absolute demonstration of their sporiferous nature can only be had by seeing them germinate. All bacterial cells may pass into a resting state when for any reason growth ceases, but such cells do not possess any spore character and are only ordinary vegetative cells under conditions unfavorable to growth. Arthrospores do not exist, and this term should be discarded. Gonidia occur in the Chlamydobacteriaceæ. In *Cladothrix* these bodies escape from the sheath as swarm cells. In *Crenothrix* and *Phragmidiothrix* the contents of the vegetative cells becomes septated into Sarcina-like cubical packets, the individual cells of which finally round off and escape on the opening of the sheath as non-motile bodies which soon grow out into new threads. In *Streptothrix* the contents of the cell breaks up into a series of ovoid or round non-motile cells which escape from the sheath and grow wherever they happen to lodge.

This paper can be had from Wilhelm Engelmann, Leipsic, for 3 marks, and ought to be in the hands of every working bacteriologist.

ERWIN F. SMITH.

Ambrosia Once More.—There are two species of *Xyleborus* which bore in orange wood, one is *X. fuscatus* and the other is really undescribed, but goes well enough for the present under the name of *X. pubescens.* Both of these are associated frequently with *Monarthrum fasciatum* and *Monarthrum mali*, not only in the orange but in other hard wood trees of any kind, and even in the wood of wine and ale casks, in which they are able to propagate their "ambrosia" as well as in living or rather in dying trees. The ambrosia is very probably a dif-

ferent fungus for the different species of these beetles, even where several species occur in the same tree trunk. The ambrosia of *X. pubescens* and *X. fuscatus* is deep black in its stain, or in its later stages, and the same fungus may serve both these species; but *X. xylographus*, a cosmopolitan species frequently found in hickory, oak, beech and the like, has a very different ambrosia, which is olive-green when dry, or leaves a stain of that color in the chambers. The ambrosia of *X. celsus*, a large species found in hickory, is dark brown in stain and in the form of its conidia is entirely different from that grown by any other of the species I have mentioned.

In the orange trees injured or killed by frost the most numerous borer is *Platypus compositus*. Its ambrosia is entirely different from that of the Xylebori in the orange, and stains the chambers dark brown. Several species of Platypus are found in the Southern States in all sorts of timber, conifers and deciduous trees alike. *P. compositus* attacks all sorts of trees, including our pines.

The scolytid boring into the whortleberry, which I brought to the Department of Agriculture last fall is *Corthylus punctatissimus*. It lives in the roots of several shrubs, as hazel, witch hazel, etc. Its ambrosia leaves a deep black stain. *Corthylus columbianus*, discovered by Hopkins, makes notable black stains in Liriodendron wood.

I have had the opportunity of examining three or four kinds of ambrosia and have found them very distinct in the form and arrangement of the conidia as well as in the habit of growth of the mycelium. There is, I think, very little doubt that the different species of ambrosia are connected with certain scolytid beetles irrespective of the wood in which they make their galleries. The different genera in which I have found the food to consist of ambrosia are *Platypus*, *Xyleborus*, *Monarthrum* and *Corthylus*. It is useless to give lists of plants attacked by these ambrosia-raising beetles because most of them make their galleries and brood chambers in a great variety of trees and shrubs Some species live preferably in the roots of plants and others in the trunks or larger branches, but very few species are restricted to one or even a few kinds of timber.

I expected to learn much about the forms of ambrosia found in galleries of the different borers in orange trees, but I find on my return here this winter that a ferment has taken complete possession of all the ambrosia which I have examined, and the operations of the beetles are for the time being at a standstill. A slide of the material now lining the galleries [Feb. 20, 1896] shows only fragments of the mycelium of the fungus, and the entire field swarms with bodies like yeast (or bac-

teria?) in active eruption. These spores form dense masses and entirely fill up many of the burrows, often smothering the insects. I am curious to know whether this is really the end of a prodigious attack that has been made by the beetles during the past summer upon the dying orange trees, oaks and other timber injured by the great freezes of last winter [Dec. 27–29, 1894 and Feb. 7–9, 1895], or whether it is only a temporary condition due to the inactivity of the beetles during the winter I should not be surprised to find that it has put an end to further increase of the colonies of ambrosia-eating insects by making it impossible for them to propagate their food fungus.—HENRY G. HUBBARD, Crescent City, Florida.

NOTE.—Mr. Hubbard reports (May 13) that the beetles were finally overwhelmed by this "intruding ferment," and now believes that their depredations are usually brought to an end in this manner.—E. F. S.

ZOOLOGY.

The Feeding Phenomena of Sea Anemones.—Nagel has claimed that only the tentacles of the sea anemones were stimulated by food, while Loeb has shown that other parts of the oral disc were equally sensitive. To settle which was correct Dr. Parker made his experiments on our common *Metridium marginatum.*[1] When the animal is expanded, carmine dropped on the tentacles is gradually carried outwards by the ciliary action until it is dropped outside the disc. In this there is at first only a slight muscular action, and then the tentacles are quiet as before. If, however, a bit of crab-flesh be dropped on the tentacles, these are stimulated much more. They now gradually bend inward, and the flesh, carried toward the tips of the tentacles like the carmine, is dropped inside the lips of the mouth. Experiments showed that this stimulation was produced by the juices of the meat, sugar, quinine, meat and picric acid—all substances with taste—as well as filter paper, rubber, etc., produced no stimulation. Each tentacle was stimulated alone. Between the tentacles and the lips is an intermediate zone in which no stimulation occurs. Bits of crab meat dropped upon it remain quiet for a time, and then gradually move outwards. Dr. Parker was not able to explain with certainty how. When, however,

[1] Bull. Mus. Comp. Zool. XXIX, No. 2, 1896.

the meat reached the tentacles, these were stimulated, and acted just as before.

The region of the lips was different in its action. At either end (sometimes at only one) is the ciliated siphonoghlphe, and here the ciliary action is constantly directed inward, and all matters placed upon them were invariably carried inward—paper, meat, quinine, sand, all acting alike. At both siphonoghyphes the current was the same—directed inward. The rest of the oral area between the siphonoghyphes is also ciliated, but here the current is normally outward. Sand or carmine dropped here was carried outward, across the intermediate zone and over the tentacles as before. If, however, a bit of meat be placed here, it at first starts outward, then stops and moves in toward the mouth, thus indicating, as did other experiments, that there was a reversal of the direction of ciliary action. Stimulation of one side in this way did not cause reversal in the other lip. Again, indifferent substances and crab-meat placed near each other moved in different directions, thus indicating that the reversal affected only a small area and not the whole of the lip. Filter paper alone was passed outward; filter paper soaked in crab juice was swallowed. No such ciliated areas occurred on the column, and this region did not react in any way to food stimuli.

The Relation of Myrmecophile Lepismids to the Ants.—The relation of the numerous forms of animal life found in ant hills (and therefore myrmecophilous) to the owners of the hills is varied. It has long been well-known that the plant-lice found in the hills have a relation to the ants nearly analoguous to that of the cow to man. They are retained and cared for by their owners for the liquid that they exude from their bodies when tickled by the "milker's" antennæ.

Certain staphylinids also exude a substance of which the ants seem to be fond, and in return are fed by the ants. As a consequence of this symbiotic relation, Wasmann has pointed out that the palpi of the staphylinids have become more or less noticeably reduced in size, thus indicating some degree of dependence upon the ants. In the case of *Claviger testaceus* found in the ant hills in the neighborhood of Paris, this dependence is so complete, according to Janet,[2] that the beetles perish upon being separated from the ants. To this sort of symbiotic relation the name-coiners have applied the term myrmecoxeny.

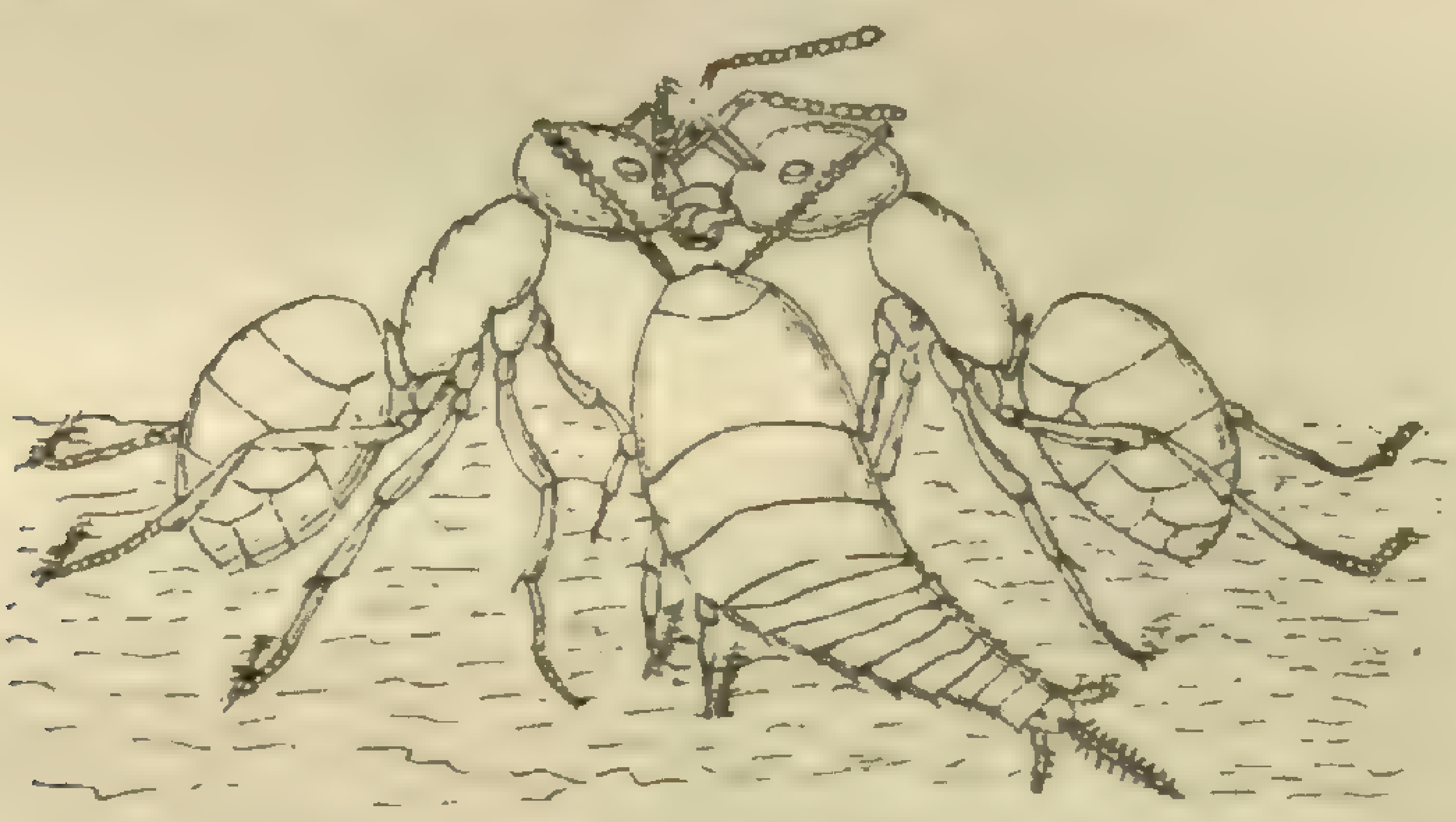
In addition to these myrmecoxenous forms there are those that like *Myrmedonia funesta* capture and devour either the ants themselves or

[2] Comptes Rendus, CXXII, 799–802.

their young, and must in consequence bear the term myrmecophagous. Then there are nematode internal and acarid external parasites. Then there are forms, that like the isopod *Platyarthrus hoffmanseggi* of Europe, flourish in and upon the detritus of the hills without molesting or being molested by the ants, a mode of life denominated synæketic.

Lastly, come the lepismids, living what Janet calls a myrmecocteptic life. Notwithstanding the name, the relation of the lepismids as told and illustrated by the above named writer is peculiar, and withal rather interesting. His experiments were performed with *Lepismina polypoda* Grassi, captured along with a colony of *Lasius umbratus* Nyl.

Some twenty-one of the lepismids were separated from the ants, and fed upon a mixture of honey, sugar, flour, and yolk of egg. At the end of two years and six months only nine remained in good condition. These willingly eat the drops of food presented to them upon the



points of fine pincers. Those left and reared along with the ants were much more active than the separated lot, running incessantly among the ants, always seeming to avoid remaining quiet in their presence. Sometimes they were pursued, but owing to their superior agility were able to escape. In the conditions of the artificial nest, however, where places of safety were doubtless fewer than under natural conditions, they were often captured. Two days after the beginning of the experiment five Lepismid cadavers were noted. In order to save the rest the colony was given a new nest, where certain places would be less frequented by the ants. These the Lepismids found, and in them remained quiet; but as soon as a single ant made its appearance, they scampered precipitously away.

When the ants were fed with their customary supply of small drops of honey, the Lepismids, by their agitation, manifested that they had become aware of the proximity of very desirable food. Meanwhile, the ants that had discovered it, gorged themselves to fulness. Then, returning to the neighborhood of their companions, who had not found

the supply, seemed to be requested by the latter to "give me some;" a request that did not seem to be refused. Soon pairs of ants became locked together mandible to mandible, the one giving, the other receiving, a drop of honey. As quickly as a lepismid perceived this condition of affairs, he rushed in between the pair and intercepted the drop or a portion of it in its passage, and then retreated precipitately, but only to treat another pair in a similar manner, and so on until his hunger was appeased. Lepisma then is not in the ant hill for an exchange of services, like some of the staphylinds; nor to be "milked," like the aphid, nor to be a common parasite, nor a common thief; but is there as more or less of a wary freebooter.—F. C. K.

Lipophrys a Substitute for Pholis.—In my article on the application of the name *Pholis* to the gunnels, I find a note was omitted, replacing the homonymous name of the blennioid genus. As the latter will be left without a proper name on account of the preoccupation of the one it has so long borne by the gunnel, a new one will be requisite for the blennioid genus, then, I propose the designation Lipophrys (λιπ, indicating want or absence; οφρύς, eyebrow)[3] in illusion to the absence of the superciliary cirri, and its type is the common *Blennius pholis* (Linn) of Europe.

I have given the family name *Pholididæ*, because there are some who will not retain *Xiphidion* on account of the existence of a prior *Xiphidium*, and therefore would not adopt the family name derived from that genus. If, however, the latter is retained, it would be better modified as *Xiphidiidæ*.—THEODORE GILL.

Blind Batrachia and Crustacea from the Subterranean Waters of Texas.—From an artesian well, 188 feet deep, recently bored at San Marcos, Texas, there were expelled more than a dozen specimens of a remarkable batrachian, together with numerous crustaceans. The latter are described by Mr. Benedict, and the batrachian by Dr. Stejneger.

The crustaceans comprise numerous shrimps (one new species, *Palæmonetes antrorum*, a lesser number of Isopods of a new genus (Cirolanides), and a very few Amphipods.

All the species are white, blind and have unusually long, slender feet and antennæ.

The Batrachian, for which Stejneger creates a new genus, is described under the name *Typhlomolge rathbuni*. It belongs to the family Proteidæ, and is more nearly allied to Necturus than to Proteus.

[3] In analogy with λίπο-βλέφαρος, without eyelids, and λίπογληνος, without eyeballs, or sightless.

Like the crustaceans, it is blind. The most remarkable external feature is the length and slenderness of the legs. In commenting on this peculiarity, Dr. Stejneger says: "Viewed in connection with the well-developed finned swimming tail, it can be safely assumed that these extraordinarily slender and elongated legs are not used for locomotion, and the conviction is irresistible that in the inky darkness of the subterranean waters they serve as feelers, their development being thus parallel to the excessive elongation of the antennæ of the crustaceans."

The gills are external, its color nearly white, having the upper surfaces densely sprinkled with minute pale gray dots, and its total length measures 102 mm. (Proceeds. U. S. Natl. Mus., Vol. XVIII, 1896.)

Lungless Salamanders.—Following up the observations of Dr. H. Wilder, certain tailed Batrachia examined by Dr. Einar Lönnberg with reference to their possessing functional lungs have brought to light the following facts: *Desmognathus auriculatus Holbr.* and *Plethoden glutinosus* Green exhibit no trace of either lungs or larynx. A median longitudinal groove is the only remaining rudiment of the aditus ad laryngem. The transverse laryngeal muscles are well developed in *Plethodon glutinosus*, as is also the median narrow strip of connective tissue at which the muscles insert themselves.

Manculus quadridigitatus has no trace of lungs, larynx or aditus ad laryngem, and, although the laryngeal muscles are well developed, the connective tissue between the muscles is very feebly developed. The median strip of connective tissue forming a point of insertion for the laryngeal muscles can be seen. This species exhibits the most reduced rudiments of the laryngeal apparatus of the specimens under observation.

Amblystoma opacum possesses rudimentary lungs and a small aditus ad laryngem. The author regards the lungs as rudimentary, because they are so very small and narrow, measuring about 9 mm. in length and 1½ mm. in width at the broadest place. It is probable that the function as respiratory organs in conjunction with some other organ, either the skin or "la cavité bucco-pharyngienne," as Camerano has found to be the case with *Spelerpes fusca.*

The theory suggested by the author to explain the reduction and oss of lungs in these animals is stated as follows: When these salamanders lost the gills and increased in bulk, the small and not very composite lungs were insufficient for respiration, so that the bucco-pharyngeal cavity (? together with the exterior integument) even from the beginning had to play a certain part. In some of the forms the

respiratory capacity of that cavity increased more rapidly than that of the lungs, which is the easier to understand, as the air breathed must first pass through that cavity, and because the cavity is rather large. When this capacity had developed to a certain extent, the lungs were no longer needed, and gradually atrophied from disuse.

All the salamanders examined lead a more or less terrestrial life; but the peculiar characteristic of reduction of lungs is not confined to terrestrial forms. (Zool. Anz. XIV, Bd., No. 494, 1896.)

Batrachia Found at Raleigh, N. C.—*Necturus maculatus.* Water Dog. This species is caught by anglers in the spring, and seems scarce, as I have only seen eight specimens so far, none of which measured over 7½ inches in total length. Some of them were evidently breeding females.

Amblystoma opacum. Marbled Salamander. Common. They lay their eggs in dry season under logs on the edges of dried-up pools, and the eggs hatch out quickly when the pools fill up again from rain; whether they do this in wet seasons I do not know. Sometimes the larvæ are very abundant, sometimes very scarce. This winter, after a dry autumn, they are abundant. Last winter, after a wet autumn, I found difficulty in securing any. The eggs are laid in October and November.

Amblystoma punctatum. Quite rare here.

Plethodon glutinosus. Viscid Salamander. Very common under rotten logs in woods.

Manculus quadridigitatus. Tolerably common. This species enters the water in December to breed, and retires to dry land again about February. It seems entirely terrestrial, except when breeding. I took nearly full grown larvæ in May, 1895.

Spelerpes bilineatus. Striped Salamander. Common. This salamander is found in the water, breeding from December to March; the larvæ first appear in May, and do not attain their full growth till a year or more afterwards. Except in the breeding season I believe it to be entirely terrestrial.

Spelerpes guttolineatus. Tolerably common. Found mostly in or around rocky springs or on the edges of rocky brooks, or of the larger streams. They can be taken containing eggs in November; but I have never seen any larvæ that had any sign of belonging to this species.

Spelerpes ruber. Red Triton. Aquatic, though like the next species sometimes found under logs not far from the water. Judging from the varying size of larvæ taken at the same time of year, I think it proba-

ble that this species spends at least one whole year, and possibly two, in the larvæ state.

Desmognathus fusca. Brown Triton. Found in all brooks, and is very common. The larvæ attain the adult condition in a shorter time than those of *Spelerpes bilineatus*, as though they are both hatched about the same time; the larvæ of this species complete their metamorphosis in the autumn or winter following their birth, being then only about one-half the size of larvæ *Spelerpes bilineatus* of the same age.

We get specimens of very varying coloration; some being nearly black, some very light.

Diemyctylus viridescens. Newt. Common in weedy pools.

Amphiuma means. Rare. I know of eight adults and twenty-two larvæ having been taken here, all being two-toed specimens.

Bufo americanus. Common Toad. Very abundant. Breeds in spring and summer.

Scaphiopus holbrookii. Last May I collected fifty breeding in a pool only a few yards from my house; in every case the grasp of the male was inguinal. The cry was not much louder than that of the common toad. I have occasionally dug them out of the ground.

Hyla versicolor. Common.

Hyla pickeringii. Abundant. Breeds in March and April.

Chorophilus feriarum. Abundant. Breeds in February and March. I have never seen this species except at the breeding season.

Acris gryllus. Cricket Frog. Abundant. Active all the year round except in the severest weather. This species breeds from April through most of the summer.

Engystoma carolinense. This species is very abundant in the breeding season, which is in July and August, and possibly the two preceding months. Have never seen any except when breeding; I think they are nocturnal.

Rana pipiens. Leopard Frog. Abundant. Breeds in March.

Rana clamata. Spring Frog. Common.

Rana catesbiana. Bull Frog. Not as common as the preceding two breeds in February and March.

Rana palustris. Pickerael Frog. Rare. Only four specimens so far. —C. S. Brimley.

The Frilled Lizard.—The report that the Frilled Lizard (*Chlamydosaurus kingii*) inhabiting the tropical parts of the Australian continent, is in the habit of running erect on its hind legs, receives confirmation from W. Saville Kent. Specimens in captivity were

seen by him to run thirty or forty feet at a stretch, in an erect position on their hind legs, and when after resting momentarily on their haunches, to resume a running course. The conformation of the hind foot is such that when running only the three central digits rest upon the ground. Consequently the track made by this lizard in passing erect over wet sand would correspond with such as are left in mesozoic strata by various Dinosauria (Nature, Feb., 1896). Mr. Kent suggests affinities with the latter order; but these do not exist, as Chlamydosaurus is a typical Lacertilian. It is not the only lizard that progresses on its hind legs, as Mr. Francis Sumichrast pointed out several years ago that a species of the Iguanid genus Corythophanes found in Mexico has the same habit.—(Ed.)

The Palatine Process of the Mammalian Premaxillary.—While engaged in the study of the comparative anatomy of Jacobson's Organ, Mr. R. Broom came across some interesting facts in connection with the palatine process of the mammalian premaxillary, which he puts on record in the Proceeds. of the Linnean Soc., N. S. W., Vol. X, 1895. From his observations he concludes that the os paradoxum in Ornithorhyncus, the anterior vomer (Wilson) in Ornithorhynchus, the anterior paired vomer in fœtal Insectivora, etc. (Parker), the prepalatine lobe of vomer in Caiman (Howes), and the vomer in Lacertilia and Ophidia (Owen, Parker, etc.), are homologues or synonyms of the process under discussion. He therefore suggests the name prevomer, to cover all the designations which the different forms of this ossification has received. (Proceedings of the Linnæan Soc. of N. S. Wales).

New formation of nervous cells in the Brain of the Monkey, after the complete cutting away of the occipital lobes.—It is known that the noviformation in the nervous cells in the nervous centres and above all in the brain has not yet received a definite solution. There has been made, however, a number of researches on this important question, but the contradictory results arrived at, have not as yet advanced our knowledge on this subject. On the contrary, the conclusions arrived at by M. G. Marinesen, presented to the Society of Biology in 1894, are that the cells and nervous fibres of the nervous centres do not grow again after their destruction.

In pursuing his studies on the physiology of the occipital lobes, M. Alex. N. Vitzou has discovered the presence of cells and of nervous fibres in the substance of noviformation, in the Monkey, two years and two months after the complete cutting away of the occipital lobes. The entire extirpation of these lobes results, as is known, in a total loss of

sight in both monkeys and dogs. The experience of the author, concerning this point agrees with that of M. H. Munck and confirms his conclusions. The later researches of different scientists have confirmed the facts which he demonstrated.

Repeating the experiment of total extirpation of the two occipital lobes of monkey, February 19, 1893, M. Vitzou noticed that during the fourth month the animal commenced to perceive persons and objects, but with great difficulty. At the end of fourteen months, the ability to perceive was greatly increased. The monkey could avoid obstacles, which he could not do during the first months following the operation.

On the 24th, of April, 1895, Mr. Vitzou repeated the operation upon the same animal. After denuding the skull he found the orifices of trepanation closed by a mass of rather firm connective tissue. On lifting this mass with care, to his astonishment and that of the assistants standing about him, he found the entire space which had formerly been occupied by the occipital lobes completely filled with a mass of new formed substance. This he proceeded at once to examine.

A portion was taken from the centre of the mass closing the orifice of trepanation, and another from the posterior part of the new formed substance found in the skull. Employing both the rapid method of Golgi and Ramon y Cajal, and the method of double coloration with hematoxyline of Erlich and eosine in aquous solution, M. Vitzou demonstrated the presence of pyramidal nervous cells and of nerve fibres. The nerve tissue was present in large quantities and the nerve cells less numerous than in the occipital lobes of the adult animal, but their presence in the new formed mass was constant.

In brief the conclusion from the preceding experiment is that the new substance occupying the place of the occipital lobes, was of nerve nature, and that it was due to a new formation of cells and of nerve fibres in the brain of the monkey. Here is a fact, says the author, which demonstrates the possibility of regeneration of nerve tissues in the brain, as well as, what was previously known, that active nutrition is maintained in the rest of the organ.

Moreover, we find in the presence of cells and nerve fibres in the new formed mass an explanation of the fact concerning the betterment, although slight, of the sense of sight. This explains also contradictory facts presented by different scientists, in the case of partial extirpation of the brain followed by an amelioration of the functions lost during the first operation.

M. Vitzou adds that the monkey having been subjected to a second operation lost the sight from both eyes for three months and a half, at

the end of which time he gave signs, although somewhat uncertain, of recovering his vision. The animal is well cared for in order that the author may continue his observations for some time to come; then later, he will be sacrificed in order that a complete study may be made of the new formation. (Revue Scientif. 1895, p. 406.)

ENTOMOLOGY.[1]

Domestic Economy of Wasps.—Much attention has recently been given to the biology of wasps. One of the most interesting accounts is that of M. Paul Marchal[2] summarized in the Annals of *Magazine of Natural History.* The investigator studied the earth-burrowing wasps (*Vespa germanica, V. vulgaris*). The fully-formed nests contain small and large cells, the latter constituting two or more of the lowest combs, while the others make up the six to ten upper combs. The large cells, built only by the workers in August, may, at an early period, receive indifferently either females or males, the former being either queens or very large workers, the latter always in small proportion; after the first of September these cells are entirely set apart for the queens, so that in October no males are to be found in them.

The small cells, from the time that the laying of eggs for males has begun, contain indifferently up to the end of the season either workers or males. The proportion of males in the combs of small cells decreases from below upwards, with this remarkable exception—that if there be a mixed comb containing both large and small cells, the small cells are influenced by the proximity of the large cells, and contain very few males.

The beginning of the period for laying males coincides very nearly with the time of appearance of large cells, early in August. The curve which represents their production rises suddenly in an almost vertical manner to reach its maximum; it then descends gradually with or without oscillations to the end of the reproduction. The queen takes a prominent part in this great production of males, because the laying workers have already long since disappeared, whilst the young male larvæ are still to be found in great numbers in the nest.

The queen has then (at least after the early days of September) the power to determine with certainty the female sex of the eggs which

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

[2] *Comptes Rendus,* t. cxxi, pp. 731–734.

she lays in the large cells; on the other hand, she lays indifferently either female or male eggs in the small cells. One can only admit in order to explain this remarkable fact, the principle of the theory of Dzierzon, based upon the fecundation, because if the production of males were due, for example, to the influence of season, it is evident that the eggs laid at the same epoch in the large cells would become male just as much as the others. In order to interpret all the facts, M. Marchal thinks this theory should be modified, by allowing the intervention of another factor than the will of the queen, and continues: We will admit, then, that after her first deposit of eggs, exclusively those of workers, which lasts until the first of August, the reflex which brings about the contraction of the seminal receptacle at the moment of the laying of each egg is no longer produced with the same energy, and that therefore the eggs can be laid without being fecundated; thence the almost sudden appearance of males corresponding to the relative state of inertia of the receptacle. Then it is that the workers building the large cells give the queen a choice between two distinct classes of the alveoli, and she, stimulated by the presence of the large alveoli, which seem to possess the power of rendering her reflexes more energetic, will concentrate from that time all her energies upon them and will lay only fecundated eggs and females. The modification thus introduced into the theory is important because it replaces the voluntary act of the queen by a passive one. The queen does not deposit males and females at will; but there comes a time when she cannot do otherwise than deposit males, because of the relative inertia of her receptacle.

M. Marchal finds that the laying of eggs by workers is normal in August to a small extent, and that it is greatly increased in case the queen is removed or stops laying.

Circulars on Injurious Insects.—A valuable series of circulars on injurious insects is being issued by the United States Division of Entomology. In each, one of the more important pests is discussed, its method of work, distribution, life-history, natural enemies and remedies being clearly described. Recent issues include circulars 9 to 15, with the following titles: Canker-worms, by D. W. Coquillet; The Harlequin Cabbage Bug, by L. O. Howard; The Rose Chafer, by F. H. Chittenden; The Hessian Fly, by C. L. Marlatt; Mosquitoes and Fleas, by L. O. Howard; The Mexican Cotton-Boll Weevil, by L. O. Howard, and Shade Tree Insects, also by Mr. Howard.

Gypsy Moth Extermination.—The last Report of the Massachusetts Gypsy Moth Commission shows that decided progress has been made in checking the pests. About $130,000 was spent during 1895. In commenting on the policy of State control of the pest, Prof. C. H. Fernald writes:

The value of the taxable property in this State is $2,429,832,966, and an appropriation of $200,000 is a tax of less than one-twelfth of a mill on a dollar. A man having taxable property to the amount of $5,000 would have to pay a tax of only 41 cents and 6 mills. This beggarly sum of money would make but a small show in the work of clearing gypsy moth caterpillars from an infested $5,000 farm, while in the uninfested parts of the State the land owners would be paying an exceedingly small premium to the State to insure them against the ravages of the gypsy moth. This premium on a $1,000 farm would be 8⅓ cents, and for fifty years it would amount to only $4.16⅔ cents. This protection would extend not only to farmers and owners of forest lands, but also to residents in villages and cities who own lots with trees and shrubs on them, and to vegetation wherever grown within the limits of our Commonwealth.

Entomological Notes.—Messrs Howard & Marlatt publish, as Bulletin No. 3, of the United States Division of Entomology, an elaborate discussion (80 pages) of The San José Scale: Its Occurrences in the United States, with a Full Account of its Life-history and the Remedies to be used against it.

In reporting[3] on the 1895 experiments with the Chinch-Bug diseases, Prof. F. H. Snow says that the year's experience corroborates the conclusion of former years that *Sporotrichum* is ineffective unless the weather conditions favor its development.

In Bulletin 36, of the Hatch Experiment Station of Massachusetts, Messrs Fernald and Cooley discuss the imported Elm Leaf Beetle, the Maple Pseudococcus, the Abbot Sphinx and the San José Scale.

Some potato insects are discussed by Prof. H. Garman in Bulletin 61 of the Kentucky Experiment Station.

Mr. M. V. Slingerland continues the excellent entomological bulletins from the Cornell University Experiment Station. Recent issues deal with Climbing Cutworms (Bulletin 104), Wireworms and the Bud Moth (107) and the Pear Psylla and Plum Scale (108).

In Bulletin No. 43, of the Minnesota Experiment Station, Prof. Otto Lugger discusses Insects Injurious in 1895. The Bulletin covers about

[3] Fifth Report of Experiment Station of the University of Kansas. Lawrence, 1896.

150 pages with sixteen plates, and shows that a large amount of work has been done. The entomological department has a special annual appropriation of $5,000 which enables it to carry on extensive field experiments.

In the December, 1895, Bulletin of the Tennessee Station, Chas. E. Chambers discusses the Chinch Bug.

Mr. Frank Benton's admirable Manual of Instruction in Apiculture, issued as Bulletin No. 1, New Series of the United States Division of Entomology, is being most cordially welcomed by the bee keeping fraternity.

EMBRYOLOGY.[1]

Morphology of the Tardigrades.[2]—R. v. Erlanger has published the results of his observations on the early development of *Macrobiotus macronyx* Dujardin. The division of the egg is total and equal, segmentation resulting in the formation of a long oval blastula with the segmentation cavity located nearer the posterior, more pointed pole. Regular gastrulation takes place, with the cells of both ectoderm and entoderm at the anterior more flattened, pole considerably larger than those posterior to the blastopore, this difference being noticable throughout the entire development. The embryo bends ventrally and the entoderm becomes constricted into two sections, the anterior, the germ of the œsophagus together with the sucking stomach and the posterior, the germ of the true stomach. The ectodermal cells of the anterior and ventral walls increase in number and size, representing respectively the starting points of the eyes and ventral nerve chain. The hind gut, extending dorso-ventrally, represents the third division and is in open communication with the blastopore. In the ensuing stage the blastopore becomes closed and later the true anus breaks through in the same place.

Up to this stage the embryo has consisted of but the two primary germ layers. The mesoderm develops as paired cœlomic pouches from the Archenteron, the first pair appearing at the posterior end of the embryo forming the fourth segment, the second pair in the anterior end giving rise to the first segment, the third pair in the second segment

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts reviews and preliminary notes may be sent.

[2] Morph. Jahrbuch., Bd., XXII, 1895.

and the fourth pair in the third segment. In addition to these the first pair of mesodermal pouches, right and left of the first pair of appendages divide and give rise to a pair of head pouches. The gonad develops as a dorsal evagination of the archenteron between the second and third segments and later pushes itself forward into the region of the second segment.

There develops further, in the region between the stomach and midgut an unpaired accessory sexual gland and at the same stage there is developed a larger pair of evaginations of the midgut which the author designates as the midgut glands. The salivary glands develop as ectodermal invaginations of the head segment. The author does not consider the musculature, the nerves, or the transformation of the cœlomic pouches, reserving those points for another paper.

After a careful consideration of historical and comparative points, in which he discusses the results and views of the various authors who have published papers on the tardigrades and presents his own ideas on the questions concerned, the author, in closing, hopes that what he has contributed to a knowledge of the morphology of the tardigrades will be sufficient to give them a place at the bottom of the arthropod stem. He does not maintain that the tardigrades represent the stem form of the arthropods but that they have branched off early and at the very bottom of the arthropod phylum and in many respects developed partially, but a considerable number of primitive characters remain which seem to show that they are transitional forms to other phyla.

In a second paper by the same author[3] the earlier embryonic stages of *Macrobiotus macronyx* Duj. are described as follows.

Contrary to condition found in the terrestrial tardigrades, in the species studied, the males are equal to the females in number. The males are smaller by half than the females, the latter appearing brownish-yellow in color owing to the eggs in the ovary which in a ripe condition attains a considerable size. The author was unable to distinguish a copulatory apparatus as described by Graff and the manner of fertilization precludes the existence of such an organ. The female withdraws her body into the chitinous envelope so that the hinder part is clear as far as the second pair of appendages and the eggs are extruded into this cavity through the anus. The hinder end of the chitinous shell of the female is turned in for a short distance, forming a short tube.

During copulation the female moves about dragging the male clinging to her back. The male deposits the spermatazoa near the posterior

[3] Biologisches Centralblatt., 15.

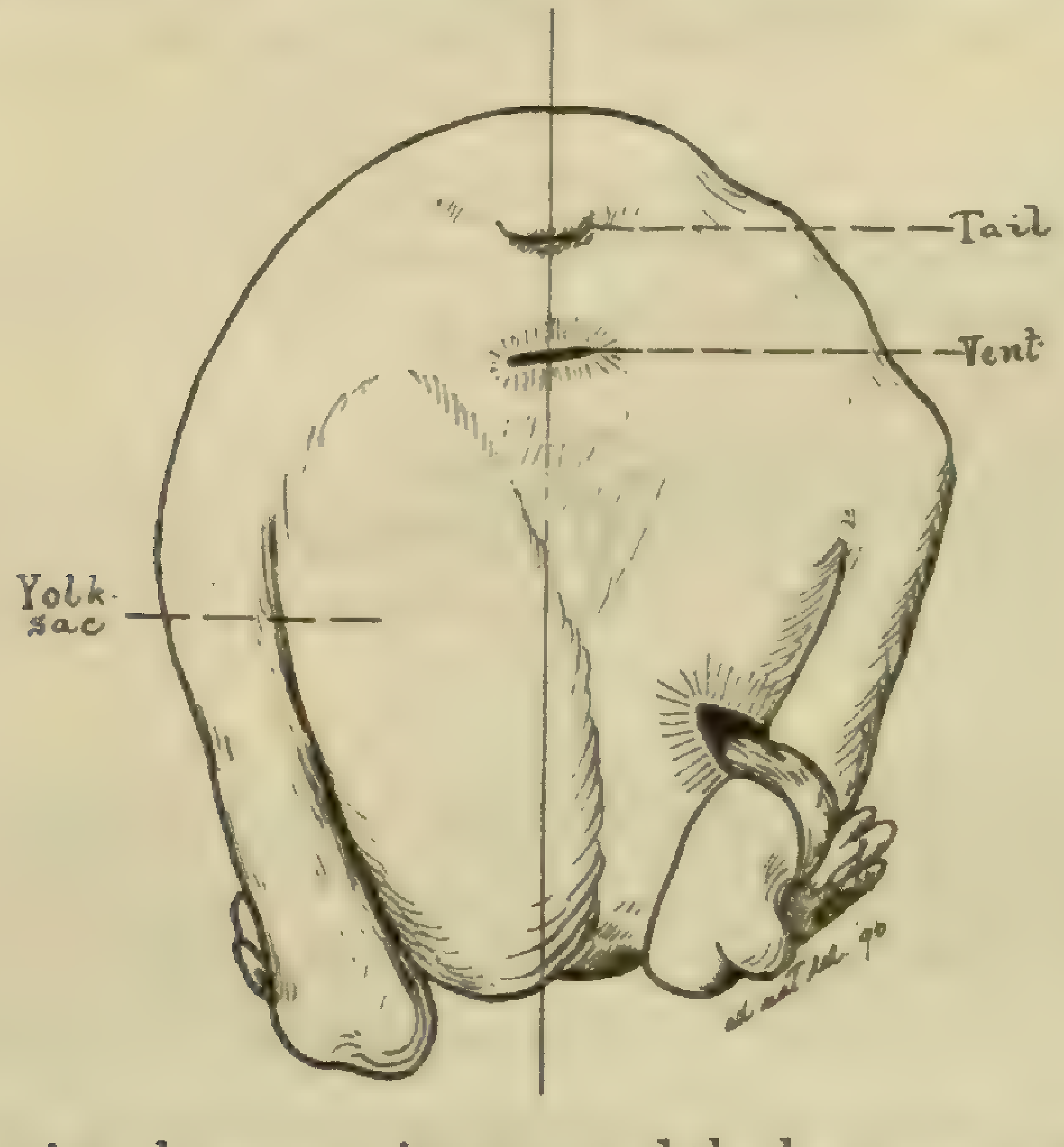
end of the female and they are sucked in through the tube at the posterior end of the female by a sort of pumping motion maintained by some peculiar muscular action on the part of the female.

The maturation stages viewed externally and in section present the usual phenomena with one peculiarity, the formation of *four* polar globules instead of three, the second globule extruded dividing in the same manner as the first, thus giving rise to the additional body.

The first division of the egg presents a two cell stage, the second division, a three cell stage, the third division a four cell stage followed by the eight, sixteen and thirty-two cell stages. In the four cell stage only two of the four cells are in contact, the former being somewhat oblique to the long axis.

The egg membrane is a product of the egg itself, and probably derived from the alveolar layer. In the young the appendicular glands, opening through tubes between the claws are much larger than in the adult and consist in great part of the cœlomic pouches. In contrast to the terrestrial tardigrades *Macrobiotus macronyx* does not revive after desiccation.—F. D. LAMBERT.

An abnormal chick.—In a brood of chicks there occurred one case in which there was an opening in the abdominal wall on the right side through which extended what seemed to be a loop of the intestine. The loop ligatured the tibio-tarsus just above the ankle joint.



Upon dissection, however, it appeared the loop was nothing more nor less than a region of the yolk sac constricted by twisting. The

(smaller) portion of the yolk—sac thus constricted off was darker and had begun to undergo decay. The opening in the abdominal wall is nearly oval, 9 mm. long by 7 mm. broad. The edges are smooth as if about a natural opening.

A glance at the accompanying figure will reveal a certain amount of asymmetry, due mostly to the position of the yolk sac.

The youngster was hatched alive. How long he might have lived, or whether the opening would have healed cannot be said, as the owner did not give it a chance.

In all probability the condition just described was caused by some inadvertent movement of the embryo, thus displacing the yolk sac or a part of it.—FRANCIS E. LLOYD.

PSYCHOLOGY.

A Study in Morbid Psychology, with some Reflections. —When Descartes uttered his famous aphorism, "*Cogito, ergo sum*," he reasonably flattered himself on having made an irrefutable proposition. But the "abysmal depths of personality" are not as easily sounded as the great French philosopher imagined. Certainly *something* thinks; but is it one consciousness or more that is represented in one human brain? The celebrated experiments of Professor Janet, of Havre, led to the discovery of no less than three distinct personalities in his patient, Madam B., and the no less noted cases of Félida X. and of Louis V.[1] show one or more personalities controlling the same brain. And there are epileptiform and hypnotic states where all the functions of civilized society are discharged without the consciousness of the ordinary primary self.

I will now proceed to describe the case, which forms the main subject of this article—that of Ansel Bourne, a carpenter and itinerant preacher of Rhode Island. The experiences of Ansel Bourne are amongst the most curious to be met with in the annals of morbid psychology. Whether the symptoms of this case are due to epilepsy —"masked epilepsy"—or post-epileptic mental disturbance, they are equally worthy a study.

Ansel Bourne, who is of New England parentage, was born in New York City, July 8, 1826, and worked steadily at his trade as a carpen-

[1] NOTE.—See article "Double Consciousness," in the Dictionary of Psychological Medicine, edited by D. Hack Duke, M. D., where cases are given, including those of Louis V. and Félida X. (cases 4 and 7).

ter till his thirty-first year. At that time, as the result of some strange experiences ending with the direct promptings of a hallucinatory (?) voice, he gave up his trade, and spent some thirty years as an itinerant preacher or "evangelist." At the time of his extraordinary seizure he was about 61 years old, and, except for some mental disturbance caused by the objection of his second wife to his work as an itinerant preacher, he seems to have been in excellent health.

On January 17, 1887, he went from his home in Coventry, Rhode Island, to Providence, in order to draw money from the bank to pay for a farm he had arranged to buy. He left his horse at Greene Station in a stable, expecting to return that afternoon from the city. He drew out of the bank $551, and paid several bills, after which he went to his nephew's store, 121 Broad Street, and then started to go to his sister's house. This was the last that was known of his doings at that time. He did not appear at his sister's house, and did not return to Greene, where his horse remained for about three weeks, till it was taken away by Mrs. Bourne.

On Thursday, Jan. 20, the following paragraph appeared in the *Bulletin*, of Providence, R. I., the information having been given by police:

A MISSING PREACHER.

"This morning, Mrs. Bourne, the wife of Ansel Bourne, of Greene Station, called at police headquarters and reported that her husband had been missing since Monday last. Rev. Mr. Bourne is quite widely known as an evangelist, and during the past twenty-five years he has carried on his religious work in various parts of the United States. For some years, it is said, he has been subject to attacks of a peculiar kind, which rendered him temporarily insensible, and on some occasions he has remained in an unconscious state for many hours. . . . . . Mr. Bourne was in Providence on Monday, but he did not return to his home, and has not been heard of since."

Notwithstanding the publicity given to the fact of his disappearance, no tidings whatever were received of him till about March 14, eight weeks later. The account of the morning of March 14 was furnished to Dr. Weir Mitchell (one of the many medical men interested in this extraordinary case) by Surgeon-General L. H. Read, who was summoned to examine Ansel Bourne on the morning of March 14, soon after he regained his ordinary waking consciousness.

It appears that Ansel Bourne arrived at Norristown, Pa., about Feb. 1, 1887, *i. e., two weeks after* his disappearance from Providence, R. I. Under the name of A. J. Brown he rented a store room at 252

Main Street, from Mr. Pinkston Earle, and divided the room into two by means of curtains. The rear portion of the room he filled with furniture and used as a "general living" apartment, sleeping and preparing his meals there. The front portion of the room he stocked with miscellaneous goods, toys, confectionery, etc. These he purchased and paid for in Philadelphia, which he visited each week with the purpose of replenishing his stock. He fastened a sign to his window reading A. J. Brown. Had he known Latin enough to say so he might truly have exclaimed, "Cogito ergo sum!" only the particular ego which was thinking would unhesitatingly have called itself A. J. Brown!

The room which he rented was part of a house in which the Earle family were dwelling, but although they came into daily contact with "Mr. Brown," there was nothing in his manner or proceedings which suggested anything peculiar. He was quiet in his behavior, precise and regular in his habits, and paid his bill promptly. He was especially punctual in the closing of his store at 9 P.M. on ordinary week days and at 10 P.M. on Saturdays. He attended the Methodist church on Sunday, and on one occasion, at a religious meeting, he related an incident he said he had witnessed on a steamer years previously, on the passage from Albany to New York, and his remarks were thought particularly relevant to the point under consideration. In short, none of the persons who had any dealings with him conceived any suspicion that he was in any unusual condition.

On the morning of Monday, March 14, about five o'clock, he heard, he says, an explosion like the report of a gun or a pistol, and waking, he noticed there was a ridge in his bed not like the bed he had been accustomed to sleep in. He noticed the electric light opposite his windows. He rose and pulled away the curtains and looked out on the street. He felt very weak, and thought he had been drugged. His next sensation was that of fear, knowing that he was in a place where he had no business to be. He feared arrest as a burglar. He says this is the only time in his life he ever feared a policeman.

The last thing he could remember before waking was seeing the Adams Express wagons at the corner of Dorrance and Broad Streets in Providence, on his way from the store of his nephew in Broad Street to his sister's residence in Westminster Street, on January 17.

He waited to hear some one move, and for two hours he suffered great mental distress. Finally he tried the door, and finding it fastened on the inside, opened it. Hearing some one moving in the next room, he rapped at the door. Mr. Earle opened it and said, "Good morning, Mr. Brown." B.: "Where am I?" E.: "You're all

right." B.: "I'm all wrong. My name is'nt Brown. Where am I?" E.: "Norristown." B.: "Where's that?" E.: "In Pennsylvania." B.: "What part of the country?" E.: "About 17 miles west of Philadelphia." B.: "What time in the month is it?" E.: "The 14th." B.: "Does time run backwards here? When I left home it was the 17th." E.: "17th of what?" B.: "17th of January." E.: "It is the 14th of March."

Mr. Earle thought Mr. "Brown" was out of his mind, and said that he would send for a doctor. He summoned Dr. Louis H. Read, to whom Mr. Bourne told the story of his doings at Rhode Island, and how he remembered nothing between the time of seeing the express wagons on Dorrance Street and waking up that morning March 14th. "These persons," he said, "tell me I am in Norristown, Pennsylvania, and that I have been here six weeks, and that I have lived with them all the time. I have no recollection of ever having seen one of them before this morning." He requested Dr. Read to wire to his nephew, Andrew Harris, at 121 Broad Street, Providence, R. I. Dr. Read telegraphed:" Do you know Ansel Bourne? Please answer." The reply came: "He is my uncle. Wire me where he is, and if well."

Later this nephew came up to Norristown, sold the goods in the store by auction, and settled up the business affairs of "Mr. Brown," who, as Ansel Bourne, travelled back with him to Rhode Island. Dr. Read adds, in the account of the case, which he furnished to Dr. Weir Mitchell; "He said he was a preacher and farmer, and could not conceive why he should have engaged in a business he knew nothing about and never had any desire to engage in. When asked about his purchasing and paying for goods, and paying freight bills, he said he had no recollection of any such transactions."

The family with whom he lived say that after the occurrence of that morning he was greatly changed. He was annoyed at any reference to his store, and never entered it afterwards. He became despondent, took no food, was unable to sleep, and became greatly prostrated, both physically and mentally.

Whether or no this was a case of "masked epilepsy," no one familiar with the peculiarities of the hypnotic state can fail to see the likeness between the experience of Ansel Bourne and that of patients who have purposely been kept for considerable periods under the influence of hypnotic suggestion. Such patients, when aroused from the hypnotic trance have never any recollection of the time which has elapsed since they were "put to sleep," although in the interim they have been carrying on the ordinary business of life as if the whole "ego" were acting.

Early in 1890, Professor James, of Harvard, hearing of the case, conceived the idea that if Mr. Bourne could be hypnotized, a complete history of the whole incident might be obtained from him whilst in the hypnotic trance. The circumstances had naturally left a painful and perplexed impression on Mr. Bourne; he was anxious to have any light possible thrown on his strange experience, and he readily acquiesced in the proposals made for hypnotism.

Here, it must be noted, that *no amount of suggestion, however strongly urged or frequently repeated, ever succeeded in merging the consciousness of "Albert Brown" in that of "Ansel Bourne;"* the one personality was absolutely separated from the other.

Ansel Bourne came to Boston on five consecutive days, May 27–31, and during that time Professor James and Mr. Hodgson obtained from him, in the "deep" hypnotic state, the following detailed account of his doings during the eight weeks from January 17 to March 13, 1887.[2]

He said that his name was Albert John Brown, that on January 17, 1887, he went from Providence to Pawtucket in a horse-car, thence by train to Boston, and thence to New York, where he arrived at 9 P. M. and went to the Grand Union Hotel, registering as A. J. Brown. He left New York on the following morning and went to Newark, N. J., thence to Philadelphia, where he arrived in the evening, and stayed for three or four days in a hotel near the depot. He then spent a week or so in a boarding house in Filbert Street, about No. 1115, near the depot. It was kept by two ladies, but he could not remember their names. He thought of taking a store in a small town, and after looking round at several places, among them Germantown, chose Norristown, about twenty miles from Philadelphia, where he started a little business in five cent goods, confectionery, stationery, etc.

He stated that he was born in Newton, New Hampshire, July 8, 1826 [he was born in New York City, July 8, 1826], had passed through a great deal of trouble, losses of friends and property; loss of his wife was one trouble, she died in 1881; three children living, but everything was confused prior to his finding himself in the horse-car on his way to Pawtucket; he wanted to get away somewhere, he did'nt know where, and have rest. He had six or seven hundred dollars with him when he went into the store. He lived very closely, boarded by himself, and did his own cooking. He went to church and also to one prayer-meeting. At one of these meetings he spoke about a boy who

[2] Professor Janet, of Havre, discovered accidentally that by inducing a deeper condition of hypnotic trance, a personality can be "tapped" which would otherwise be unknown.

had kneeled down and prayed for the passengers on a steamboat from Albany to New York.[3]

He had heard of the singular experience of Ansel Bourne, but did not know whether he had ever met Ansel Bourne or not. He had been a professor of religion himself for many years, belonged to the "Christian" denomination, but back there everything was mixed up. He used to keep a store at Newton in New Hampshire, and was engaged in lumber and trading business; had never been previously taken up with the business which took him to Norristown. He kept the Norristown store for six or eight weeks. How he got away from there was all confused; since then it has been a blank. The last thing he remembered about the store was going to bed on Sunday night March 13, 1887. He went to the Methodist church in the morning, walked out in the afternoon, stayed in his room in the evening and read a book.[4]

During the enquiry, one of the most remarkable phenomena is the utter failure of suggestion to combine the Bourne with the Brown state, thereby demonstrating that suggestion is *not* the principal factor in hypnotism. I will give two instances:

At 11.45 A. M., May 31, Mr. Hodgson hypnotizes Bourne, and, after a couple of minutes, says, "What's your name? It's Bourne, is'nt it?" "No, it's Brown." Mr. Hodgson wakes him up and tries the same experiment again, with the same result: at the first touch of trance he is Brown. Other experiments were made on succeeding days to connect the two personalities, but vainly. On July 7, at 10 P. M., Ansel Bourne was entranced by Mr. Hodgson who *tells him he will remain Ansel Bourne after being hypnotized.* In vain; he passes at once into the Brown state. Mr. Hodgson then enumerates the chief events of Bourne's life, telling "Brown" that he is "Bourne," and that he remembers these events. This is repeated several times, and Mrs. Bourne and Professor James reiterate the same circumstances. "Brown," however, reaches nothing more than a faint remembrance of the year of his birth, of his first marriage and of the death of his first wife. It seems doubtful, though, if these remembrances were not connected with the "Brown" state, because "Brown" always gave the date of his birth (though not the place) correctly, and remembered he had had a wife who was dead.

[3] An experience of his real life.

[4] A detailed account of the questions and answers in this enquiry is given, but would take up too much space here.

It would take too long to recapitulate all the evidence collected by Mr. Wm. Romaine Newbold, Lecturer on Psychology in the University of Pennsylvania, in verification of Ansel Bourne's statements whilst in trance. At the Kellogg House, where "Brown" stayed for about two weeks before going to Norristown, he was well remembered by the colored waiter Jackson, and by Mrs. Kellogg. They described him as a very quiet man, who said he was a carpenter and came from "down east, somewhere." Every day he used to go out and look out for a suitable place to begin business in. After a while he came one day and said he had found just the place for him, and that was Norristown. Then he bought goods for the store he intended to open there, all of which goods he left in Jackson's care. "Seemed perfectly himself," and never gave any reasons for wishing to commence business here. Afterwards, Jackson and Mrs. Kellogg had seen accounts in the papers and recognized the man there referred to as the man who had stayed with them. They thought, however, he had become crazy after leaving them, but it never occurred to them that there had been anything wrong with him whilst with them.

I will now give an account, as briefly as I can, of the curious experiences which befel Ansel Bourne when he was about thirty years of age; experiences which were accounted for by the medical man who attended him (Dr. Thurston, of Westerly, R. I.) as the results of sunstroke, and by the people in the village where he lived as "Wonderful Works of God."

Ansel Bourne, as already stated, was of New England parentage, and, up to the age of thirty-one, was a hard working carpenter who, from being a member of the Baptist church, became a "convinced atheist," not of the aggressive sort, but "silent and stubborn." It must be noted that this "atheism" in a man of scanty education must have been of the shallowest sort, and that beneath the surface lay depths of Calvinistic ancestry and training. He had conceived a rooted aversion for a sect calling itself the "Christian" church, and for one of its ministers who was his near neighbor.

In August, 1857, he had several attacks of sickness, brought on possibly by working in extremely hot weather, and these attacks culminated in a fit of unconsciousness which lasted from Sunday, the 16th of August, till the following Tuesday, when he became conscious of his condition, but remained in a critical state for some days. The next two months were passed in renewed attempts to work, and fresh attacks of illness, though of a less serious character than those of August. On Sunday, the 25th of October, he spent the day and evening at his

own house playing cards—a horrible crime for the Calvinist conscience which was lying ready to revenge itself!

[5] On the 28th of October, Ansel Bourne started for the village of Westerly, and was noticed by some neighbors to be walking fast, as though feeling quite well. He was conscious of no unusual feelings till the thought came vividly into his mind that he ought to go to meeting (*i. e.*, to church). Mentally he enquired, "Where?" The inner voice replied, "To the 'Christian' chapel." To this idea his spirit rose in bitter opposition, and he said within himself, "I would rather be struck deaf and dumb forever than to go there." A few minutes after he felt giddy, and sat down on a stone by the wayside to rest. He saw an old man in the distance approaching him with a wagon, and immediately after felt as though some powerful hand drew down something over his head and face and finally over his whole body, depriving him of his sight, his hearing and his speech, and leaving him perfectly helpless. Yet, he declares, he had as perfect a power of thought as at any time in his life, and the awful choice he had made (that he would rather be deaf and dumb forever than go to the Christian chapel) came with awful significance before him. His whole mind was full of agonizing horror and dread of the God he thought he had so irretrievably offended. He was conscious of being taken up in the wagon; of being carried into a house and placed in a chair, and then of being put in bed.

Dr. Thurston, who was summoned immediately, says that on reaching his patient's bedside he "found him perfectly insensible . . . . the pupils of his eyes quite insensible of light, widely dilated and not contracting on the application of sudden and vivid light." The patient himself, however, constantly maintained that he was entirely conscious. "About him," he says, "all was as silent as though there were neither a God, nor life, nor motion in the whole, wide universe. The silence was as though the soul had been cast into a deep, bottomless and shoreless sepulchre, where dismal silence was to reign eternally." He fully acknowledged the justice of God and spurned from his soul the thought of insulting God by asking mercy for such a sinner.

Powerful counter-irritants were applied, and by Friday consciousness was partially restored for external things. He felt the posts of his bedstead and the window near, and was satisfied he was in his own house; he felt movements on the bed and recognized the caresses of his little children; then, about 26 hours after the attack, his power of sight suddenly returned. He saw his wife and a neighbor, and made signs that

[5] From an account written under the direction of Ansel Bourne.

he wanted pen and paper. An internal voice asked "if he were willing to forgive those he had injured?" and he immediately answered in the affirmative. He expressed in writing a wish to see the minister of the "Christian" church and another neighbor with whom he had been on bad terms. Both came and treated the sufferer with kindness and sympathy; and then when he was reconciled with his brother men, he felt emboldened to approach God and offered up "unutterable prayer."

A prayer-meeting was held in Ansel Bourne's house, and he wrote saying he was determined thenceforth to be on the Lord's side. On November 11th, just two weeks from the time of his seizure, he was carried to the Christian chapel, and though unable to speak or hear, he endeavored to signify his altered feelings to the congregation by standing and holding up his hands. He also wrote a very touching message to be delivered for him by the minister. He was requested by the minister, after his second visit to the chapel, to stand up in the pulpit, and here suddenly his hearing returned—in his own words, "Every manner of sound that comes from the living things of nature broke upon his ears" . . . . his tongue was unloosed instantly, and he exclaimed, in the hearing of the whole congregation, "Glory to God and the Lamb forever!" It is needless to say that this scene, and the moving exhortation from the convert which followed, caused the deepest emotion in the congregation.

From that day onward, until the 17th of January, 1887, Ansel Bourne's faculties were unimpaired. But two weeks after the restoration of his speech and hearing in chapel, he had a "vision" which commanded him to "Settle your worldly business and go to work for me." This vision came back several times in the same night, and the result of all these experiences was that Ansel Bourne became an "evangelist," and for more than thirty years went about preaching, attending at revivals and performing strenuously all the offices of an unattached minister. At the wish of his second wife, whom he married in 1882, he gave up his itinerant preaching; and he thinks the distress of mind, caused by leaving what he considered the path of duty, may have led to the strange mental experiences which I have already described.—ALICE BODINGTON.

(*To be Continued.*)

A Match-Striking Bluejay.—The note in the November, 1895, NATURALIST, concerning the striking of matches by one of the monkeys (*Cebus*) has just fallen under my notice.

It may interest the readers of the NATURALIST to know that a neighbor of mine once had a little bluejay (*Cyanocitta cristata* (Linn.)) which

had acquired the same habit, but confined exclusively to the so-called "parlor" or "popping" matches. I never knew how he acquired the habit—perhaps accidentally, by striking them with the beak or beating them against some hard substance as he did much of his food.

When given a match he always hopped to a chair-round and struck it almost directly downward, fulminate "end on," and if it did not explode at once his blows were repeated rapidly until it ignited. He would then drop it, spring away and watch it wonderingly while it burned. All matches about the house had to be kept from him. He knew them by their odor, and would tear open packages to get them out. On one occasion his mistress came in and found him with a box from which he had ignited nearly three dozen.

—James Newton Baskett.

Mexico, Mo.

ANTHROPOLOGY.[1]

Professor Holmes Studies of Aboriginal Archictecture in Yucatan.—Professor W. H. Holmes in his recent visit to the Islands on the east coast of Yucatan, the sites of Chichen Itza, Izamal and Uxmal and certain shell heaps, near Progreso (See Archeological Studies among the ancient cities of Mexico, by W. H. Holmes. Field Columbian Museum Publication 8. Chicago 1895) has presented us with a valuable and characteristically clear summary of the important architectural features of the Peninsular ruins.

Eschewing archaeological investigation in such directions as those of implements, pottery, metals, art, food, burial, etc., he fixes our attention upon the stones used in building, the manner of dressing and laying them and the purpose of completed structures. The details of this subject casually referred to by Charnay and Waldeck and in the unindexed pages of Stephens, are summed up to together with certain original observations and arranged in order, until we see the relationship, in purpose that characterizes the ruined structures in the region. No demonstration has yet been made as to the kind of tools used in carving the limestone of the facades and Professor Holmes like all previous travellers, leaves the question unanswered. Neither does he refer to Mr. McGuires' theory that the work was done with round hammerstones. But a block fortunately found at Chichen Itza, pecked on

[1] This department is edited by H. C. Mercer, University of Pennsylvania.

the surface with a pointed instrument and lined off for edge dressing with a flat edged tool, is shown as an interesting illustration (Fig. 1) of

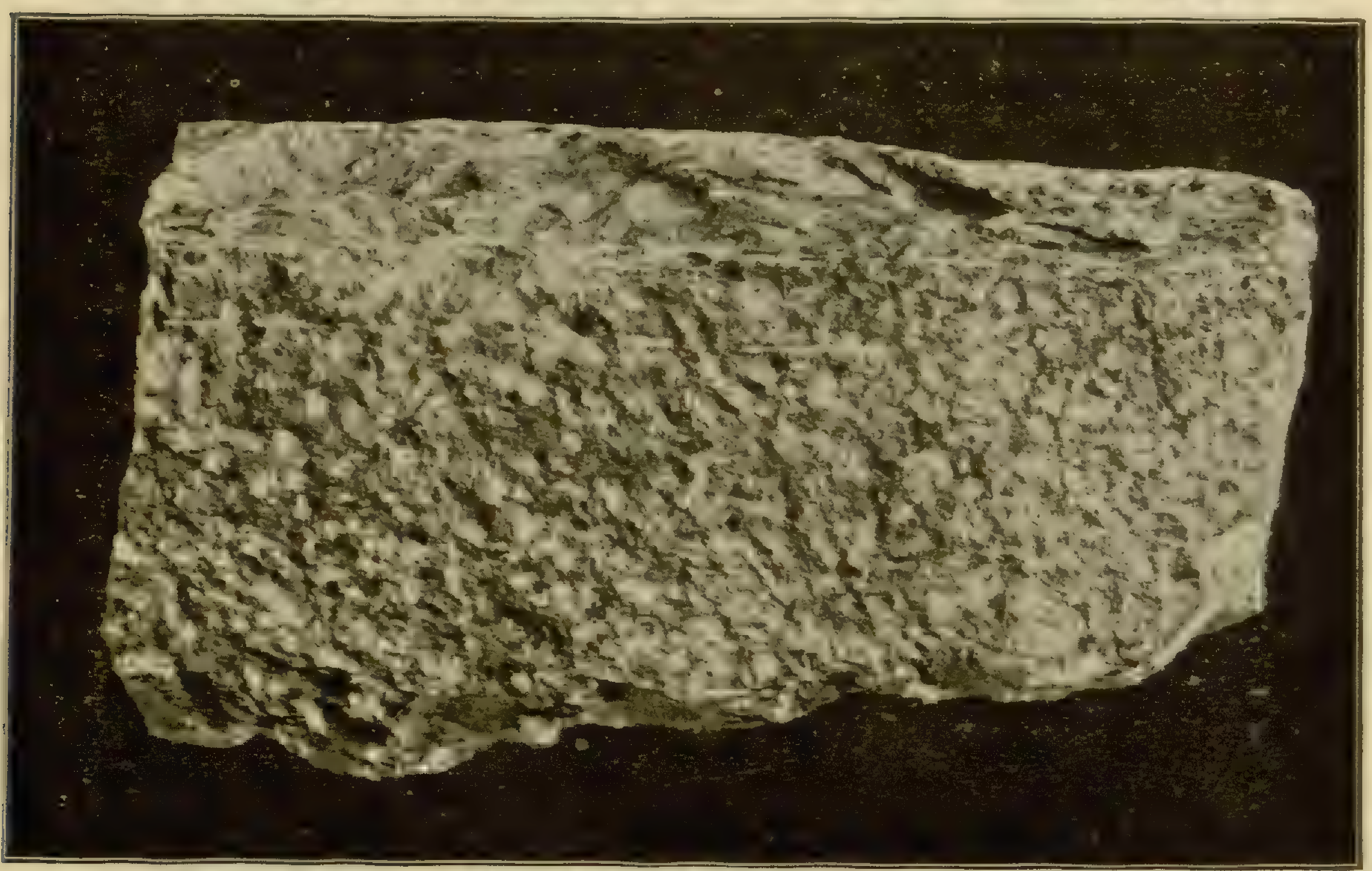


Fig. 1. Fragment of Stone from Chichen Itza, supposed to have been hewn by the ancient masons of Yucatan, the tools used are unknown, but we see the peckings of *a pointed implement* on the dressed side, and the long cuts of *an edged tool* along the upper margin.

the effect on stone of the kind of tool we are hunting for. Until we find the implement, however, we may believe on early Spanish authority, that hard copper was used, or imagine adzes and chisels of stone as we please, while we recognize with Professor Holmes the importance of ransacking the sites of quarries, where the innumerable blocks (20,000 carved on the facade of the "Governors House", at Uxmal alone) were procured.[2] Happily chosen general observations give a clearness to the whole presentation, and the delightful yet confused and complex impression of the ruins left upon the mind by the accounts of travelers becomes simple in the colder light of Professor Holmes systematic observations. The reader continually thanks him as he would thank the compiler of an index to a work of many volumes. Such characteristic general features as the ignorance of a master principle of mason craft like joint binding, the feeble grasp of the

[2] Captain Theobert Maler informed me in Ticul in 1895, that he had seen several such quarries.

PLATE VIII.

Fig. 4. Miniature portal of a small temple on the Island of Cozumel, a little entrance only 4 feet 6 inches high, to a diminutive building not over 20 feet square by 10 to 12 high resting on a terrace about 5 feet high, of the two round columns supporting the stone lintels, one is carved to represent a kneeling human figure. From a photograph by Mr. E. H. Thompson.

PLATE IX.

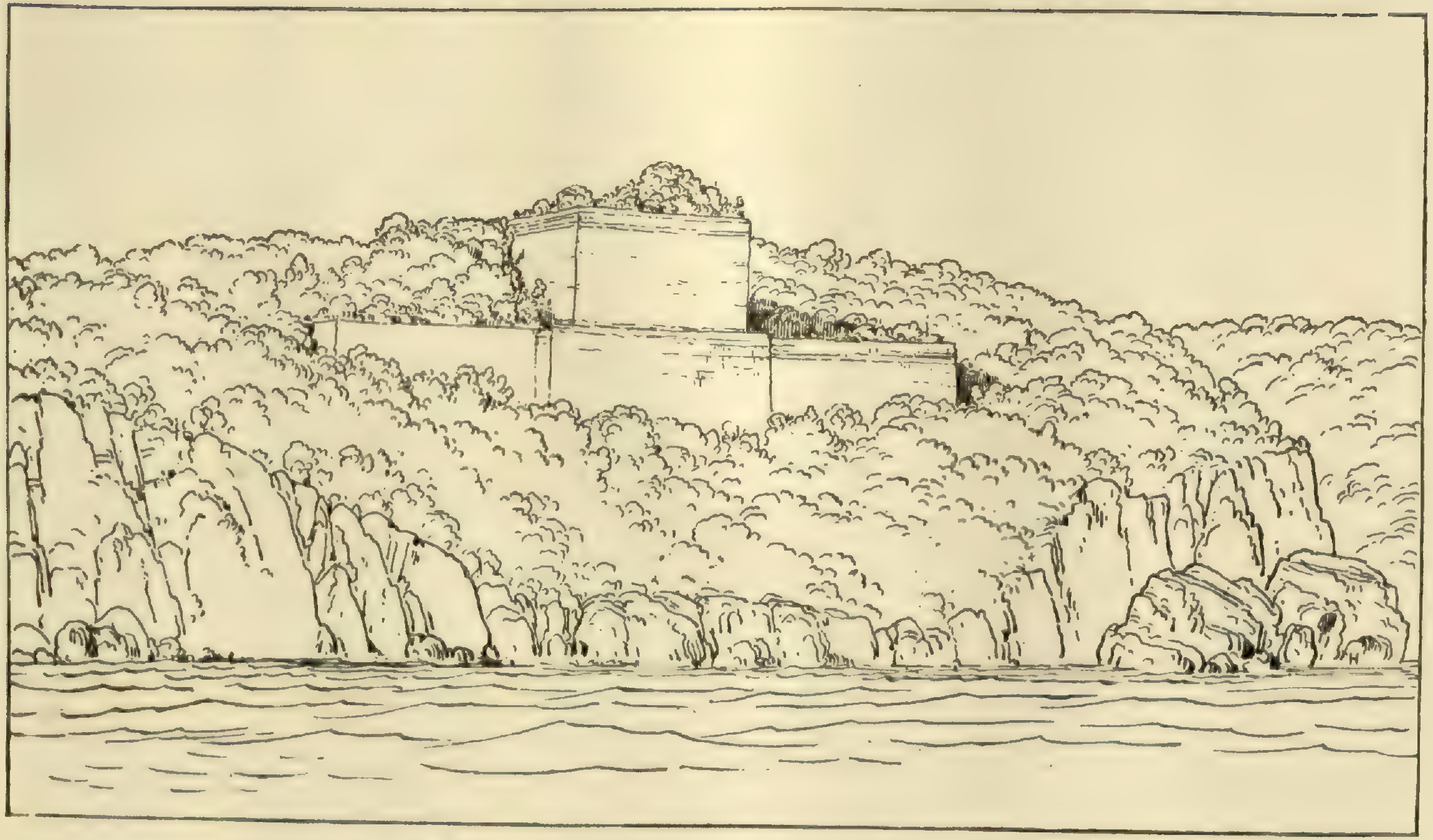


Fig. 5. Ruins of Tuloom as seen from the Sea.
The building fronts inland and its rear walls facing the water are without doors and windows.

facade upon the structure where no long stones project from the pudding like hearting within into the face, to clinch the crust to the mass, the V shape and consequent lack of catch of many of the facing

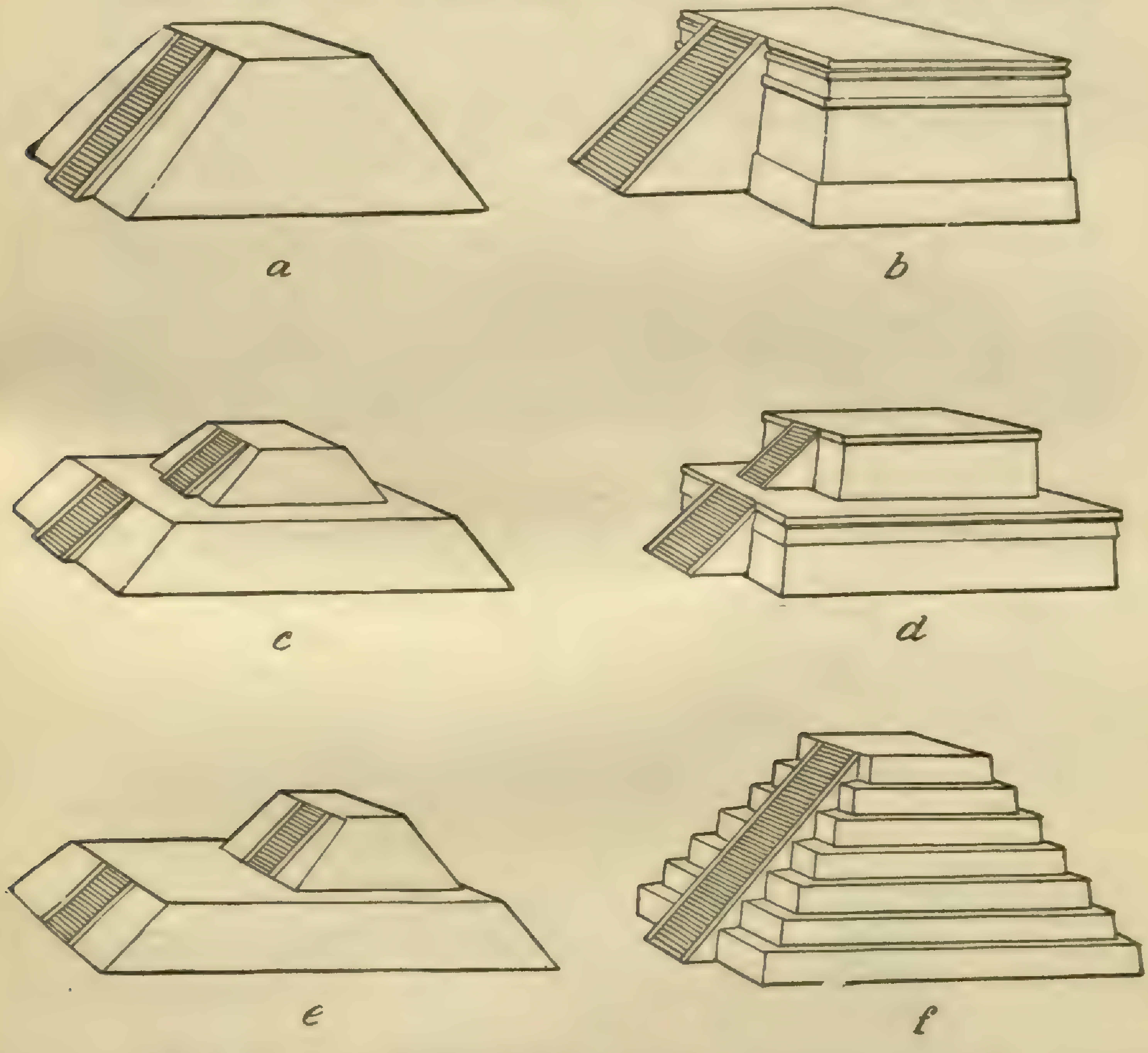


Fig. 2. Examples of Terraces and Pyramids, superstructures omitted.

stones, are dwelt upon in order, and a series of sketches disposed to catch the attention and impress the memory, show the varying forms of tumuli, (Fig. 2) the generally rectangular ground plan of buildings, (Fig. 3) and the construction of the arch by the edging in of opposing walls.

A question of much interest is touched upon when Professor Holmes in the introduction, refers to the geological age of the rock floor of the region in question, since the chance for establishing conclusions in Yucatan as to man's existence in geologically ancient times diminishes according as we learn that the Peninsula was too long under water

to count as an early human foothold. My statement (See Hill Caves of Yucatan. Lippincott, Phila., 1895, p. 21) referring to the rocks of Yucatan as of Mesozic Age, is at variance with the recent observations of geologists, while Professor Holmes says on the other hand, (p. 18): "The massive beds of limestone of which the Peninsula is formed contain and are largely made up of the remains of the marine forms of life now flourishing, along the shores. Fossil shells obtained from the rocks in various parts of the country are all of living species and represent late Pliocene or early Plistocene times, thus possibly bringing the date of the elevation of Yucatan down somewhat near that of the reputed sinking of Atlantis, some eleven or twelve thousand years ago, or not far from the period that witnessed the oscillations attending the glacial period." Though true that the peninsular limestone is largely composed of existing marine forms we learn on

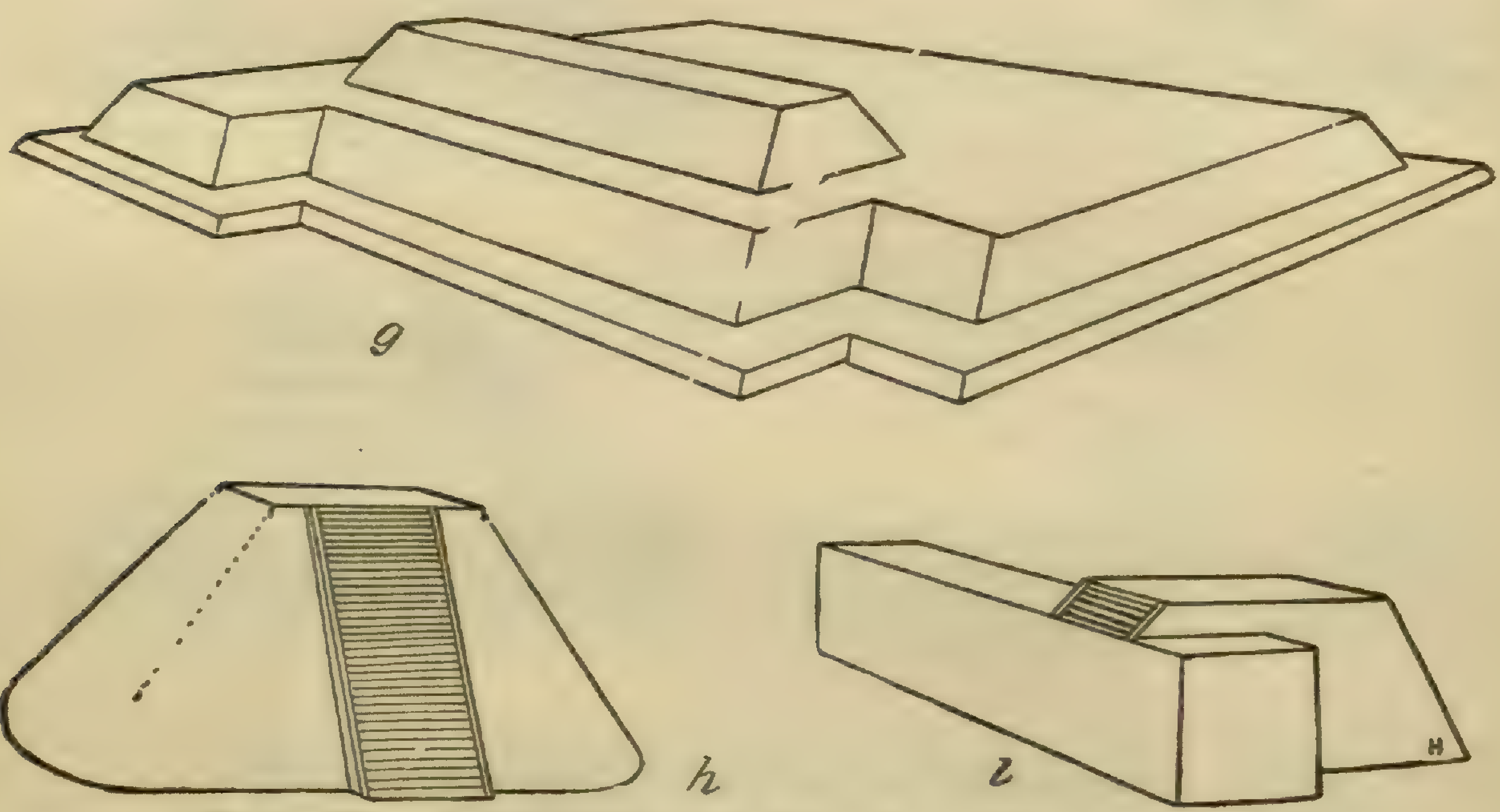


Fig. 2. Examples of Terraces and Pyramids, superstructures omitted.

closer examination that it is not entirely so, and that the shells are not *all* modern. We find that the full list of age denoting fossil mollusca collected from the rocks of Yucatan by the expedition in 1891 of the Academy of Natural Sciences of Philadelphia (See Geol. Researches in Yucatan, by Prof. Angelo Heilprin, Proc. Acad. Nat. Sci. 1891. p. 136) does not characterize the Yucatan rock as of Plistocene Age while the recent researches of geologists (Prof. J. W. Spencer makes the Niagara Gorge 32,000 years old) now tend to add to the antiquity of the Glacial Epoch. Professor Heilprin who conducted the Yucatan

expedition informs me that " the fossil shells are not *all* recent species since even the level plains about twenty to twenty five miles from the coast contain fossil mollusca (Amusium mortoni, from Cenotes near Merida, Turritella perattenuata and Turritella apicalis from R. R. cut one-half mile east of Tekanto. Ostrea meridionalis and Arca species undetermined, from a digging near Merida and Lucina disciformis) not now known to be living, and which make part of the Floridian formation (the typical Pliocene of the United States). Furthermore in the Sierra which contains the caves, a number of fossil forms have been found the determination of which is rather doubtful, but which

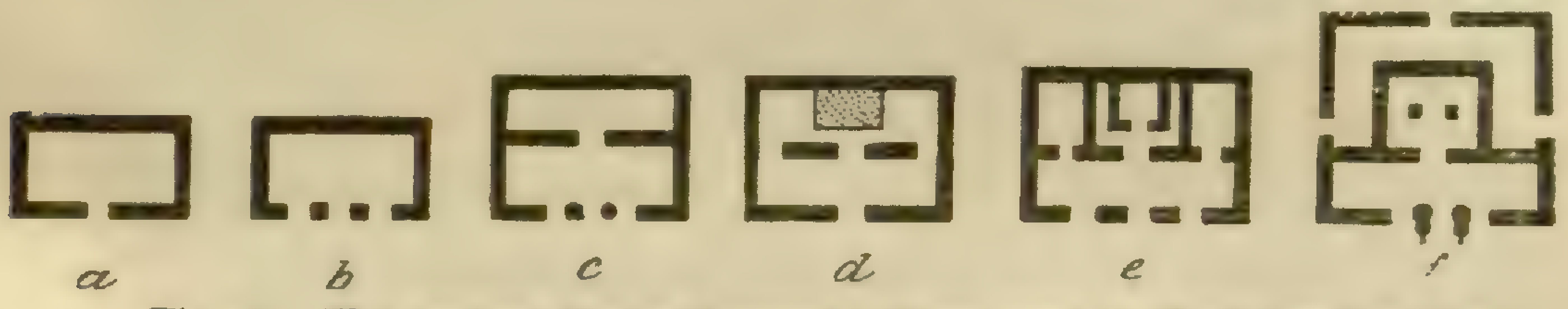


Fig. 3. The various kinds of ground plans used in Maya (ancient Yucatecan) temples.

(*a*) Single chamber building with plain door.

(*b*) Single chamber temple with wide doorway and two square columns.

(*c*) Two chamber temple with wide doorway and round columns and the Sanctuary with single plain doorway.

(*d*) Two chamber temple, the vestibule with simple doorway and the Sanctuary with three doorways and a low altar.

(*e*) Four chamber temple Palenque type, the vestibule with three entrances and two squarish piers, the Sanctuary with tablet chamber, and two small lateral chambers.

(*f*) Three chamber temple, Chichen Itza type, the vestibule entered by wide portel with two serpent columns, this Sanctuary enlarged by introducing two square columns to support the triple vault, and a long gallery with three doorways extending behind.

may be of early Pliocene or even of Miocene Age." Professor Pilsbry of the Academy of Natural Sciences and Mr. C. W. Johnson of the Wagner Institute say further after examination of the shell bearing rock specimens brought home by the Expedition above mentioned and now in the Academy of Natural Sciences, that " the shells indicate late Pliocene but by no means Plistocene Age, the presence of several characteristic Pliocene species Turritella (2 species) Fulgur rapum. Pecten eboreus Amusium mortonii, and Ostrea meridionalis preventing the possibility of the rocks being assigned to a later Epoch than the Pliocene while the fossils extinct and still existing considered together, indicate that the formation was contemporaneous with the Floridian formation of Prof. Heilprin."

In the second part of the volume a talent for lucid simplification impresses us in novel panoramic views of Uxmal and Chichen Itza, when stationed upon an imaginary height, we view the arrangement of walls and mounds clear of obscuring masses of leafage and rubbish, add to this something of the ever delightful charm of the landscape painter in sketches illustrating the course of expedition along the east coast, as we follow it from the Isle of women (Mujeres) to Tuloom, and from Cozumel to Cancun and El Meco. Looking from water to land we seem to see the tropical distance taking on its mirage like garb of coolness, and by grotesque pinnacles of rock, hear the rush of green waves upon the sands, where mysterious walls set softly in the deceitful blue allure us from the shore.—HENRY C. MERCER.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

Nova Scotian Institute of Science.—May 11, the following papers was read: Notes on the Geology of Newfoundland, by T. C. Weston, Esq., F. G. S. A., Ottawa; Phenological Observations for 1895, by A. H. McKay, Esq., LL. D., F. R. S. C., Superintendent of Education; Glacial Succession in Central Lunenburg, by W. H. Prest, Esq., Chester Basin, N. S.; On the Flora of Newfoundland, No. 3, by Rev. Arthur C. Waghorne, New Harbour, Newfoundland; Notes on Nova Scotian Zoology, No. 4, by Harry Piers, Esq.; Water Supply of the Towns of Nova Scotia—Financial, Sanitary and other Considerations, by W. R. Butler, Esq., M. E., Professor of Mathematics, Natural Philosophy and Engineering, King's College, Windsor; On the Broad Cove Coal Field, by W. H. Ross, Esq., C. E.—HARRY PIERS, *Secretary.*

Boston Society of Natural History.—The Annual Meeting was held Wednesday evening, May 6th. The following business was transacted: Reports of the Curator, Secretary, Librarian, Treasurer and Trustees; Announcement of the award of the Walker Prize for 1896; Election of Officers for 1896-97. The following paper was read: Prof. Charles S. Minot: On the Principles of the Construction of the Microtomes. There was shown a collection of the microtomes illustrating the evolution of the instrument, and also a microtome of a new model.

May 20.—The following papers were read: Prof. E. S. Morse: Man as a Tertiary Mammal; Dr. G. A. Dorsey: On the Photograph and Skeleton of Neddy Larkin, a native of Australia.—SAMUEL HENSHAW, *Secretary.*

American Philosophical Society.—April 17.—Dr. D. G. Brinton read an obituary notice of Henry Hazelhurst.

May 1st.—A Symposium on the Factors of Organic Evolution was held. Three stated papers were followed by open discussion. The papers were read by Prof. E. D. Cope, who approached the subject from the standpoint of paleontology; Prof. E. G. Conklin, who discussed it from the embryologic point of view; and Prof. L. H. Bailey, who adduced the facts of botany in support of his conclusions. Dr. D. G. Brinton discussed the papers previously read.

May 15th.—Prof. A. H. Smyth read an obituary notice of Henry Phillips, Jr. Prof. E. D. Cope read two papers, entitled Sixth Contribution to the History of the Miocene Vertebrata of N. A.; and Second contribution to the history of the Cotylosauria.

Philadelphia Academy of Natural Sciences.—May 8th.—Anthropological Section.—Papers were read by Dr. M. V. Ball on "Tattooing among Convicts." and by Dr. H. Allen on "Ethnic Bearing of the Classification of the Hand."—CHAS. P. MORRIS, *Recorder*.

The Academy of Science of St. Louis.—At the meeting of May 18, 1896, Professor C. M. Woodward presented a critical examination of some of the mathematical formulæ employed by Herbart to represent mental phenomena, in which these formulæ were criticised as inadequate. Though not considering any formulæ likely to be adequate, from the nature of the case, the speaker offered a substitute for the Herbart formula pertaining to the bringing into consciousness of a sub-latent concept through the suggestion afforded by another concept similar in some respects while differing in others.

Dr. A. N. Ravold made a report on the use in St. Louis of diphtheria antitoxine, prepared by the Health Department of the city. During the past winter, 342 cases of diphtheria had been treated with this serum, by 93 physicians. Doses of from 2.5 to 106 cc. had been administered. As a rule, the recovery was far slower when the quantity used was small than when a larger quantity was employed. Usually the serum was administered only once. In about half the cases a decided change for the better was noticeable within 24 hours, and these cases were practically cured within 48 hours, although attention was called to the fact that for some weeks the throat of a convalescent is a breeding-place for the diphtheritic bacilli, the virulence of which did not seem to be diminished by the serum treatment. Of the cases reported on, 9.06 per cent. only, died, and as a considerable number of cases were hopeless when treatment was administered, the patients dying

within 24 hours thereafter, it was considered fair to deduct these deaths from the total, which reduced the mortality to 4.6 per cent. when the serum was administered in the earlier stages of the disease. The injurious consequences of administering the serum were fully considered, but held to be practically insignificant. It was also stated that when used on persons who had been exposed to but had not manifested the disease, the serum proved an unfailing means of conferring immunity for a certain period of time. Among the advantages in the use of this serum was mentioned that of lessening the chances of secondary infection, so frequent after an attack of diphtheria.

A committee presented resolutions on the death of Dr. Charles O. Curtman, for many years a member of the Academy.—Wm. Trelease, *Recording Secretary.*

SCIENTIFIC NEWS.

The Biological Laboratory of the Brooklyn Institute of Arts and Sciences will open at Cold Spring Harbor, Long Island, July 3d, 1896 for its seventh session. As in the previous years Prof. Herbert W. Conn, of Wesleyan University is the Director. This year he is assisted by Prof. H. T. Fernald who gives instruction in Embryology, Prof. H. S. Pratt who takes charge of general zoology, Dr. D. S. Johnson, instructor in Botany, Dr. Edward L. Rice Assistant in Biology and W. H. C. Pyncheon instructor in photography. The session lasts six weeks but students, upon special arrangement, can remain longer. The Institute now possesses five buildings for the use of the laboratory, a good equipment of the apparatus necessary for collecting and for investigation and can accommodate about sixty students. The laboratory fees are as follows: The laboratory fee, including any one course of instruction, the general lectures and the use of the laboratory privileges is $20.00. For each additional course of instruction an additional fee of $5.00 is charged. The fee for the course in elementary zoology is $15.00. Board is furnished for $4.50 a week; rooms from $1.50 to $3.00 a week. The total expense for the session is thus from $55.00 to $75.00. For circulars and other information address, Prof. H. W. Conn, Middleton, Ct.

The second Annual Meeting of the Botanical Society of America will be held in Buffalo, N. Y., on Friday and Saturday August 21 and 22, 1896. The Council will meet at 1.30 p. m. on Friday, and the Society will be called to order at 3 p. m. by the retiring President, Dr.

William Trelease, Director of the Missouri Botanical Garden. The President-elect, Dr. Charles E. Bessey, Professor of Botany in the University of Nebraska will then take the chair. The afternoon session will be devoted to business. At the evening session the retiring President will deliver a public address on "Botanical Opportunity." The sessions for the reading of papers will be held on Saturday at 10 a. m. and 2 p. m. The Botanical Society of America is affiliated with the American Association for the Advancement of Science whose sessions this year begin on Monday August 24th, in Buffalo.

At the Springfield Meeting of the American Association for the Advancement of Science, it was voted that the Sectional Committee of Section E be directed to prepare a program for the meeting of the Section, and to transmit the same to the Permanent Secretary for printing and distribution not less than one month before the meeting.

It is therefore requested that members intending to present papers at the Buffalo Meeting of the Association send title and abstract of the same to the Secretary of the Section on or before July 15, 1896.

A. C. GILL, *Secretary Section E.*

Prof. S. P. Langley states that the Smithsonian Institution has decided to rent a table at the Naples Zoological Station for another period of three years, for the benefit of American students. The interest manifested by American educators and biologists in this matter has convinced the Institution that in so doing it best cooperates with the educational institutions of the United States.

About July 1st next a party under the direction of Mr. T. H. Mobley will start from Lacomb, Alberta, for a two year exploring trip through northern Canada, taking in all points of interest between Edmonton and the Arctic Sea. There are four in party, all of which are thoroughly experienced men, of whom two are naturalists.

Dr. H. Baumhauer, formerly of Lüdingshausen, goes to the University of Freibourg, Switzerland, as ordinary professor of Mineralogy, while Dr. O. Mügge, of Münster, goes to a similar position in the University of Königsberg.

Two of the honors received by Prof. Leuckart in connection with his fifty year doctor-jubilee were elections to honorary membership in the Zoological Society of France and the Russian Academy of Sciences.

Dr. G. B. Grassi, of Catania, well-known for his researches on the structure of the lower Arthropods has been called to the University of Rome as professor of comparative anatomy.

Mr. F. E. Willey, formerly of the Kew Botanical Gardens, has gone to Sierra Leone, West Africa, as director of the botanical gardens there.

Prof. O. Bütschli, of Heidelberg, has been elected president of the German Zoological Society for the years 1896 and 1897.

Dr. M. von Lenhossék has been advanced to the position of professor extraordinarius in the University of Tubingen.

Dr. Dannenberg is now privat docent for Mineralogy and Geology in the technical school at Aix-la-Chapelle.

A. Quadri, Professor of Zoology in the University of Siena, Italy, died December 25th, 1895.

Dr. G. Karsten, of Leipzig, goes to the University of Kiel as Private docent in Botany.

Mr. H. T. Wharton, a well known English ornithologist, died recently at the age of 50.

Prof. F. Chatin, has been elected Vice-President of the French, Academy of Sciences.

Dr. O. L. zur Strassen is now private docent for Zoology in the University of Leipzig.

Dr. A. Weiss has been appointed assistant in Mineralogy in the University of Greifswald.

Dr. Jean Müller, Director of the Botanical gardens of Geneva, is dead at the age of 68.

Dr. F. von Wagner, of Graz, goes to Giessen as assistant in the zoological institute.

Dr. A. N. Beketow, of St. Petersburg, has resigned from the chair of botany there.

Dr. P. Vuillemin has been appointed to the chair of botany at Nancy, France.

Dr. P. Voglino has been appointed Docent in Botany in the University of Turin.

Dr. F. Dahl, of Kiel, has gone to New Guinea, to study the flora and fauna.

Dr. P. Knuth, of Kiel, has received the title of Professor of Botany.

J. B. Wilson, botanist, of Gerlong, Victoria, died October 22, 1895.

Dr. L. Jacoby, ichthyologist, of Zürich, Switzerland, is dead.

The botanist Prof. K. Rattlef, died recently.

Dringende Bitte

Um das Erscheinen des

Botanischen Jahresberichts

möglichst zu beschleunigen, wie eine Steigerung der Zuverlässigkeit in der Berichterstattung zu erlangen, richten wir an die

Botaniker aller Länder

die dringende Bitte um gefällige schleunige Zusendung ihrer Arbeiten, namentlich auch der Sonderabdrücke aus Zeitschriften, etc.

Alle Sendungen sind zu richten an den Herausgeber.

Professor Dr. E. Koehne,
Friedenau-Berlin,
Kirchstrasse 5.

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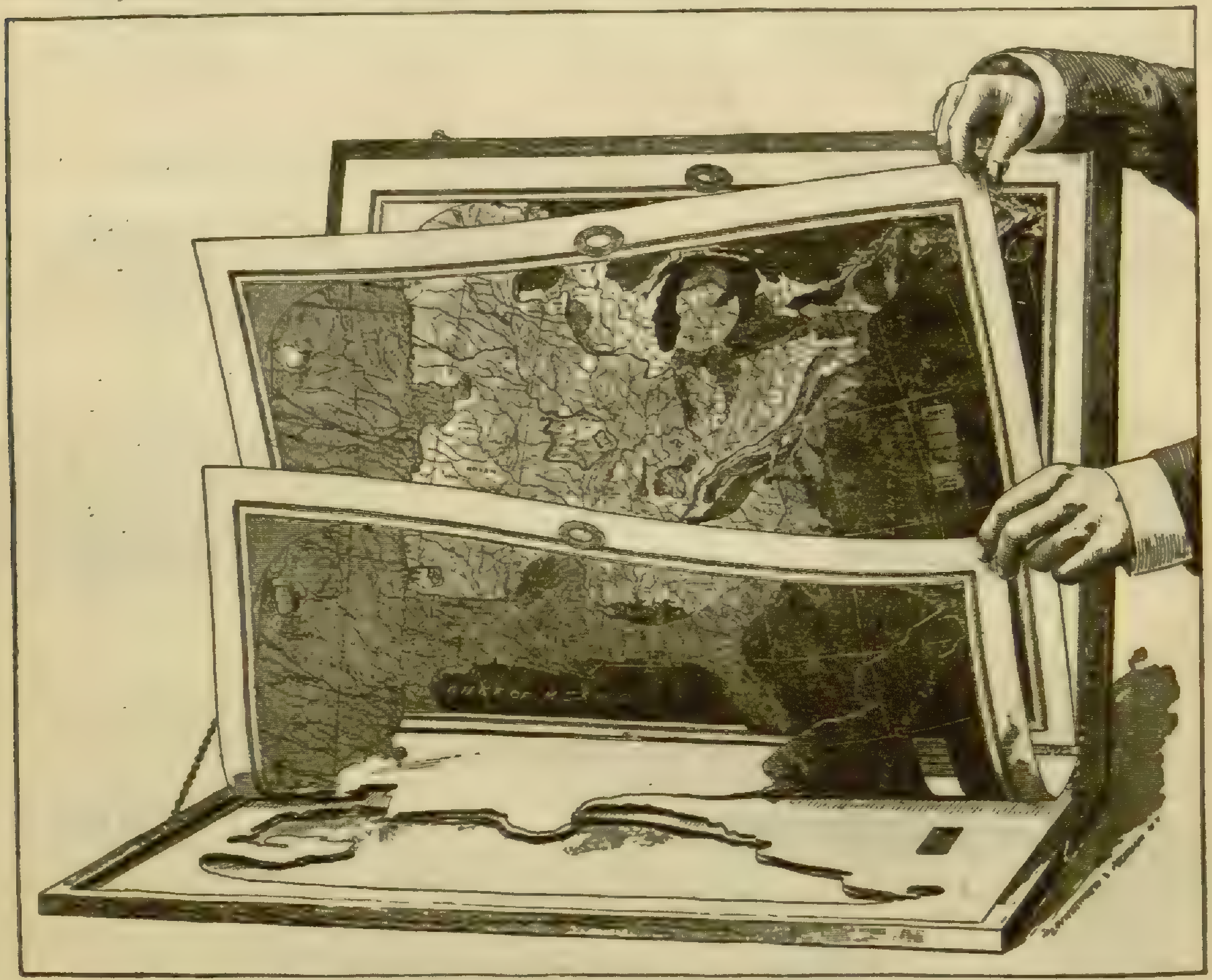

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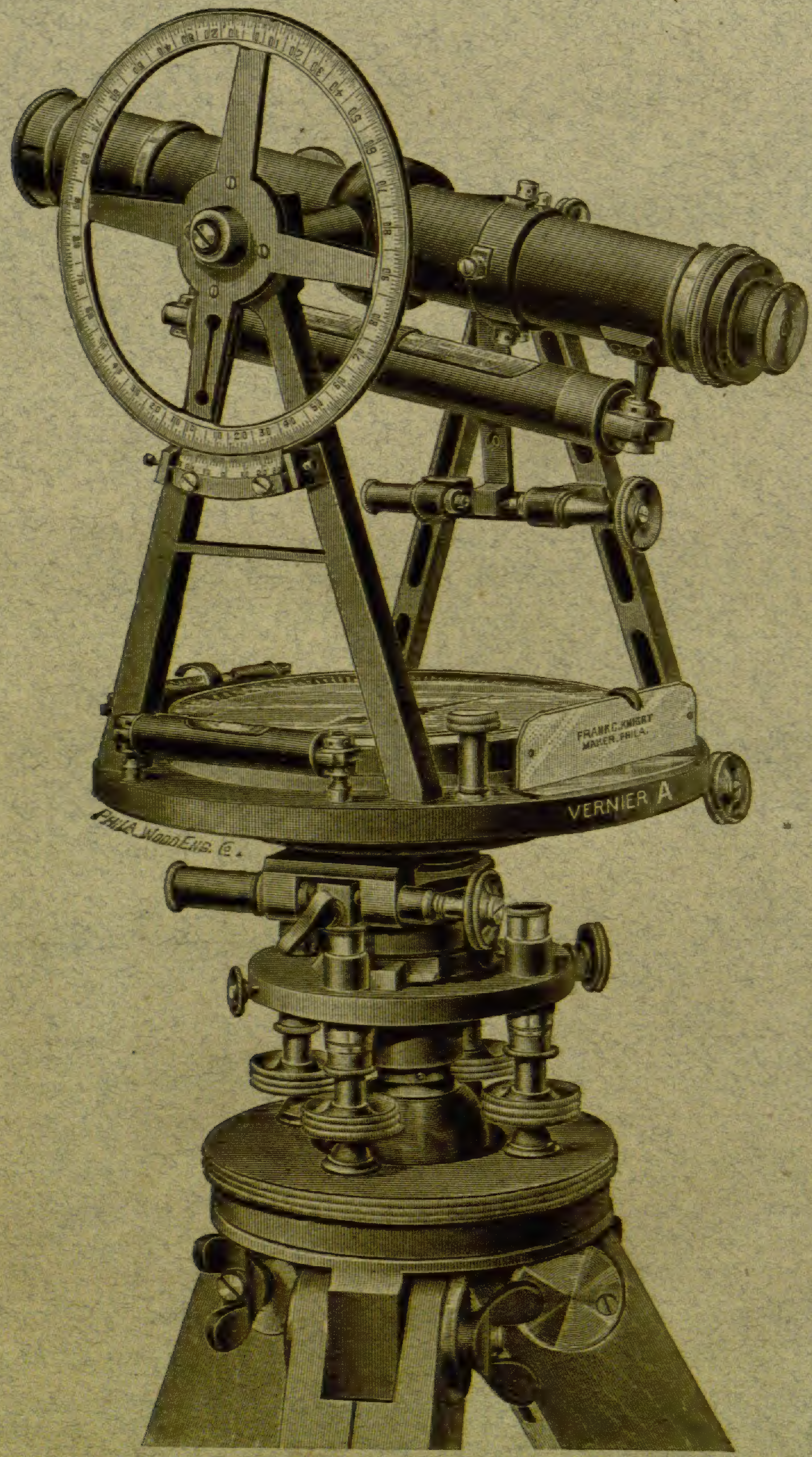

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